

PHILOSOPHICAL
TRANSACTIONS,
OF THE
ROYAL SOCIETY
OF
L O N D O N.

VOL. LXXVI. For the Year 1786.

P A R T I.



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SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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PHILOSOPHICAL
TRANSACTIONS

OF THE

ROYAL SOCIETY

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VOL. LXXVI. FOR THE YEAR 1806

PART I.



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Printed by J. JOHNSON, ST. PAUL'S CHURCH-YARD.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.



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THE President and Council of the Royal Society adjudged,
for the Year 1785, the Medal on Sir GODFREY COPLEY's
Donation, to Major-General WILLIAM ROY, for his Mea-
surement of a Base on Hounslow-Heath.

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I. *Observations on the Graduation of Astronomical Instruments; with an Explanation of the Method invented by the late Mr. Henry Hindley, of York, Clock-maker, to divide Circles into any given Number of Parts. By Mr. John Smeaton, F. R. S.; communicated by Henry Cavendish, Esq. F. R. S. and S. A.*

Read November 17, 1785.

PERHAPS no part of the science of Mechanics has been cultivated by the ingenious with more assiduity, or more deservedly so, than the art of dividing Circles for the purpose of Astronomy and Navigation. It is said, that TYCHO BRAHE

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and HEVELIUS laboured this part of their instruments with their own hands; and though public rewards have at length brought forth different methods of dividing from our best artists, which have been communicated to the Public; yet I trust it will be thought, that if any thing relative to this business remains yet behind, that may tend to furnish the ingenious artists, who are cultivating this field, with any new or curious idea upon the subject, it will be well worth communicating to this learned Society: since, if an hint, which is essentially different from any thing that (so far as I know) the Public is in possession of, be once started, and is pursued and worked upon by ingenious men, it is not possible to say, to what valuable purposes it may be converted.

This, perhaps, will better appear by taking a short review of the labours of others, from the time of TYCHO BRAHE and HEVELIUS (who did not use telescopic sights) to the present time.

The very learned, ingenious, and inventive Dr. HOOK, in his *Animadversions on the Machina Caelestis* of HEVELIUS, published in the year 1674, has given us an elaborate description of a quadrant, whose divisions were formed, and afterwards read off, by means of an endless screw, working upon the outermost border of the limb of a quadrant; which, he says, *does not at all depend upon the care and diligence of the instrument-maker in dividing, graving, or numbering the divisions, for the same screw makes it from end to end*; yet he has given us no account of any particular care or caution that he used, in preventing the same screw from making larger or smaller paces, in consequence of unequal resistance, from a different hardness of the metal in different parts of the limb; nor any method of correcting or checking the same; nor of making a screw, the

angle

angle of whose threads with the axis shall be equal in every part of the circumference; therefore the whole of this business (in which accurate *mechanists* well know consists the whole of the difficulty) he refers to the *ingenious workman*; and, in particular, to the then celebrated Mr. TOMPION, whom, he says, he employed to make his instrument, and who had thereby *seen and experienced the difficulties that do occur therein*: but was any ingenious workman now to pursue the directions of Dr. Hook, so far as his communication thereof extends, we may conclude, that he would make a very inaccurate piece of work, far inferior in performance to what the Doctor seems to expect from it*. But yet, I believe, it was the *first* attempt to apply the endless screw and wheel, or arch, to the purpose of forming divisions for astronomical instruments; for, the Doctor says himself, the perfection of this instrument is the *way* of making the divisions; that it *excels all the common ways of division*: and in the table of contents it is intitled, *An Explication of the new Way of dividing*.

This method, however, of Dr. Hook's was not laid aside without a very full and sufficient trial: for Mr. FLAMSTEED, in the *Prolegomena* of the third volume of *Historia Caelestis*, informs us, that *he* contrived the sextant, wherewith his observations were chiefly made, from his entrance into the Royal Observatory in the year 1676 to the year 1689. This sextant was first made of wood, and afterwards of iron, with a brass limb of two inches broad, by Mr. TOMPION, at the expence of Sir JONAS MOORE; the radius thereof was 6 feet 9½ inches; it was furnished with an endless screw upon its limb of 17

* This was indeed verified in an attempt upon the same plan by the Duc DE CHAULNES, published in a Memoir of the Royal Academy of Sciences at Paris, for the year 1765.

threads in an inch, and with telescopic sights*. Of this instrument Mr. FLAMSTEED gives the figure at the latter end of his *Prolegomena* before-mentioned, sufficiently large to see the general design; the whole being mounted upon a strong *polar axis* of iron, of three inches diameter.

Though, in the full description of this instrument, Mr. FLAMSTEED mentioned the Limb's being furnished with *diagonal divisions*, distinguishing the arch to 10 seconds†; yet it is pretty clear, that it had not these originally upon it; but that the dependance was wholly upon the screw divisions, when it came out of Mr. TOMPION's hands. This one may reasonably infer from the observations themselves; for the first observation, set down as taken with this instrument, being upon the 29th of October, 1676, it was not till the 11th of September, 1677, that the column which contained the *check angle* by diagonal lines was filled up; and there was also a space of time, antecedent to that last mentioned, wherein no observations are recorded as taken with this instrument, in which time the diagonal divisions might be put on; and this will be put beyond a doubt, as he says expressly, that finding, in the year 1677, that the threads of the screw had worn the border of the limb, he divided the limb into degrees himself, and drew a set of diagonal divisions‡; and then comparing the two sets of divisions together, he sometimes found them to differ a whole minute; wherefore, for correction thereof, he constructed a new table for conversion of the revolutions and parts of the screw into degrees, minutes, and seconds; and

* “—Qualem nemo, cœlo adhibens;—” Preface to *Historia Cœlest.* printed in one vol. 1712.

† *Prolegomena Histor. Cœlest.* vol. III. p. 104.

‡ *Ibid.* p. 106. “Grædus in limbo distribui ac diagonales duxi.”

which

which he applied in the observations taken in 1678.—However, notwithstanding this correction, in looking over the observations noted down as deduced each way, I frequently find a difference of half a minute; not unfrequently $40''$; but in an observation of the moon, of the 9th June, 1687, I find a difference of $55''^*$, which upon a radius of 6 feet 9 inches amounts to more than $\frac{1}{10}$ th part of an inch.

In the year 1689, Mr. FLAMSTEED completed his *mural arc* at GREENWICH; and, in the *Prolegomena* before-mentioned, he makes an ample acknowledgement of the particular assistance, care, and industry of Mr. ABRAHAM SHARP; whom, in the month of August, 1688, he brought into the observatory, as his *amanuensis*; and being, as Mr. FLAMSTEED tells us, not only a very skilful mathematician, but exceedingly expert in mechanical operations †, he was principally employed in the construction of the mural arc; which in the compass of fourteen months he finished, so greatly to the satisfaction of Mr. FLAMSTEED, that he speaks of him in the highest terms of praise ‡.

This celebrated Instrument, of which he also gives the figure at the end of the *Prolegomena*, was of the radius of 6 feet $7\frac{1}{2}$ inches; and, in like manner as the sextant, was furnished both

* Vol. I. of Hist. Cœlest. p. 343.

† “Qui mechanicoe perquam expertus, pariter ac matheos peritus.” *Prolegomena*, vol. III. p. 108.

‡ “SHARPEIUS servus meus fidelissimus, ac omnibus quidem dotibus & facultatibus erat imbutus, quæ ipsum tam subtili & difficili operi obeundo idoneum redderent.” *Prolegom.* ibid.

And on finishing the instrument, he says, “Gradus describuntur five numerantur et exculpantur, artificiosa manuali opera dicti domini SHARP, qui limbum partitus est, diagonales duxit, totumque organum absolvit et perfecit: adeo ut præstantissimi quivis artifices postquam illud conspexerunt et considerarunt, se exactius id peragere non potuisse, agnovcrunt.” *Prolegom.* p. 111.

with screw and diagonal divisions, all performed by the accurate hand of Mr. SHARP. But yet, whoever compares the different parts of the table for conversion of the revolutions and parts of the screw belonging to the mural arc into degrees minutes and seconds *, with each other, at the same distance from the zenith on different sides; and with their halves, quarters, &c. will find as notable a disagreement of the screw-work from the hand-divisions, as had appeared before in the work of Mr. TOMPION: and hence we may conclude, that the method of Dr. HOOK, being executed by two such masterly hands as TOMPION and SHARP, and found defective, is in reality not to be depended upon in nice matters.

From the account of Mr. FLAMSTEED it appears also, that Mr. SHARP obtained the zenith point of the instrument, or *line of collimation*, by observation of the zenith stars, with the face of the instrument on the east and on the west side of the wall †: and that having made the index stronger (to prevent flexure) than that of the sextant, and thereby heavier, he contrived, by means of pulleys and balancing weights, to relieve the hand that was to move it from a great part of its gravity ‡.

I have been the more particular relating to Mr. SHARP, in the business of constructing this mural arc; not only because we may suppose it the first good and valid instrument of the kind, but because I look upon Mr. SHARP to have been the first person that cut accurate and delicate divisions upon astronomical instruments; of which, independent of Mr. FLAMSTEED's testimony, there still remain considerable proofs: for, after leaving Mr. FLAMSTEED, and quitting the department above-mentioned †,

* Hist. Cœlest. vol. II. Appendix.

† Prolegom. p. 109.

‡ Mr. SHARP continued in strict correspondence with Mr. FLAMSTEED so long as he lived, as appeared by letters of Mr. FLAMSTEED's found after Mr. SHARP's death; many of which I have seen.

he retired into *Yorkshire*, to the village of *Little Horton*, near *Bradford*, where he ended his days about the year 1743; and where I have seen not only a large and very fine collection of mechanical tools (the principal ones being made with his own hands), but also a great variety of scales and instruments made therewith, both in wood and brass, the divisions whereof were so exquisite, as would not discredit the first artists of the present times: and I believe there is now remaining a quadrant, of four or five feet radius, framed of wood, but the limb covered with a brass plate; the subdivisions being done by diagonals, the lines of which are as finely cut as those upon the quadrants at *Greenwich*. The delicacy of Mr. SHARP's hand will indeed permanently appear from the copper-plates in a quarto book, published in the year 1718, intituled, *Geometry improved by A. Sharp, Philomath.* whereof not only the geometrical lines upon the plates, but the whole of the engraving of letters and figures, were done by himself, as I was told by a person in the mathematical line, who very frequently attended Mr. SHARP in the latter part of his life. I therefore look upon Mr. SHARP as the first person that brought the affair of hand division to any degree of perfection.

Some time about the establishment of the mural arc at *Greenwich*, the celebrated Danish Astronomer OLAUS ROEMER began his domestic Observatory, which he finished in the year 1715, as we are informed by his historian PETER HORREBOW, in the third volume of his works, in the tract, intituled, *Basjs Astronomiae*, published in the year 1741. In this tract is the description of an instrument, Tab. III. which not only answered the purpose of the meridian arc; but, its telescope being mounted upon a long axis, became also in reality what we now call a *Transit Instrument*; and which furnished, so far

as I have been able to learn, the first idea thereof. One end of the axis of this instrument being the center of the meridian arc, and carrying its index, M. ROEMER thereby avoided the errors arising from the plane of the mural arc not being accurately a vertical plane; and which Mr. FLAMSTEED endeavoured to check, by observing the passage of known stars nearly in the same parallel of declination; that is, passing nearly over the same part of the plane of the arc; by which he was enabled to correct or check the errors of the arc in right ascension. But it is the peculiar method in which ROEMER divided his instruments, that occasions him here to be introduced.

Though it is a very simple problem by which geometricians teach how to divide a given right line into any number of parts required; yet it is still a much more simple thing to set off upon a given right line, from a point given, any number of equal parts required, where the total length is not exactly limited; for this amounts to nothing more than assuming a convenient opening of the compasses, and beginning at the point given, to set off the opening of the compasses as many times in succession; as there are equal parts required; which process is as applicable to the arch of a circle as it is to a right line. Of this simple principle ROEMER endeavoured to avail himself.

For this purpose M. ROEMER took two stiff, but very fine-pointed, pieces of steel, and fixed them together, so as to avoid, as much as possible, every degree of spring that would necessarily attend long-legged compasses, or even those of the shortest and stiffest kind when the points are brought near together. The distance of the points that he chose was about the $\frac{1}{4}$ or $\frac{1}{2}$ of an inch. This, upon a radius of 24 or three feet, would be about 10 minutes. With this opening, beginning

at the point given; he set off equal spaces in succession to the end of his arch, which was about 75° . Those were distinguished upon the limb of the instrument by very fine points, which were referred to by a grosser division, the whole being properly numbered. The subdivision of those arches of 10 minutes each was performed by a double microscope, carried upon the arm or radius of the instrument, the common focus being furnished with parallel threads of single silk, whereof eleven being disposed at ten equal intervals, comprehending together one ten-minute division, the distance of the nearest threads became a very visible space, answerable to one minute each, and therefore capable of a much further subdivision by estimation.

The divisions of this instrument were therefore, properly speaking, not degrees and minutes; but yet, if exactly equal, would serve the purpose as well, when their true value was found, which was done by comparison with larger instruments.

Now, if it be considered, that in going step by step of ten minutes each, through a space of 75 degrees, there will be a succession of 450 divisions, dependant upon each other; if it be also considered, that the least degree of extuberance in the surface of the metal, where each new point is set down, or the least hard particle (wherewith all the base metals seem to abound) will cause a deviation in the first impression of a taper point, and thereby produce an inequality in the division; it is evident, that though this inequality may be very small, and even imperceptible between neighbouring divisions, yet among distant ones, it may and will arise to something considerable; which, in the mensuration of angles, will have the same ill tendency as in near ones. Now, as M. ROEMER has given us no

means of checking the distant divisions, in respect of each other, it is very probable that no one has followed his steps, in cases where great accuracy was required, in a considerable number of divisions. For in reality this method is likely to fall far short of Dr. Hook's; as Dr. Hook's divisions being cut in a similar successive manner, by the rotation of the sharp edge of the threads of a screw against the exterior edge of the limb of the instrument, a very slight degree of pressure will bring a fine screw of thirty threads in an inch (which he prescribes) to touch against an arch whose radius is four or five feet in more than one, two, three, or four threads at once; so that the threads supporting one another, a small extuberance, or even a small hard particle in the metal, will be cut through or removed by the grinding or rather sawing motion of the screw; and which, in regard to its contact, being in reality an edge, will be much more effectual (that is, more firm) in its retention than a mere simple point: and a repetition of the operation, from the support of the threads to each other, will tend to mend the first traces; whereas, in ROEMER's way, a repetition will make them worse; for, whatever drove forward or backward the point on first entering, will, from the sloping of the point, be confirmed and increased in driving it deeper.

When Dr. HALLEY was chosen Astronomer Royal (Mr. FLAMSTEED's instruments being taken away by his executors), Mr. GRAHAM undertook to make a new mural quadrant, about the year 1725: who, uniting all that appeared valuable in the different methods of his predecessors, executed it with a degree of contrivance, accuracy, and precision, before unknown: and the division thereof he performed with his own hand. The model of this quadrant, for strength, easy management, and convenience, has been ever since pursued as the most perfect.

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What I apprehend to be peculiar in it, was the application of the arch of 96° ; not only as a check upon the arc of degrees and minutes, but as superior thereto, being derived from the more simple principle of *continual bisection*.

To make room for this, he has entirely rejected the subdivision by diagonals, and has adopted the method of the *Vernier*; but the subdivision of the vernier divisions he, as I apprehend for the first time, measured by the turns of the detached adjusting screw, making it in fact a micrometer, by which the distance of the *set* of the instrument was to be measured from the perfect coincidence of one of the actual divisions of the limb with the next stroke of the vernier; by which means the observation could not only be read off with all the precision that the division of the instrument was capable of, but the two sets of divisions could be checked and compared with each other. Another thing that I apprehend to be peculiar in this instrument, was the more certain method of transferring and cutting the divisions, from the original divided points, by means of the *beam-compass*, than could possibly be done from a *fiducial edge*, as had doubtless been constantly the practice in cutting diagonals; for, placing the steady point of the beam-compass in the tangent line to that part of the arc where each division was to be cut, the opening of the compass being nearly the length of the tangent, the other point would cut the division in the direction of the radius nearly; and though in reality an arch of a circle, yet the small part of it in use would be so nearly a right line, as perfectly to answer the same end; all which advantages put together, it is probable, induced Mr. GRAHAM to reject the diagonals.

Soon after the completion of this quadrant, Mr. GRAHAM undertook to execute a *zenith sector* for the Rev. Dr. BRADLEY,

which was fixed up at *Wanshead*, in *Essex*, in the year 1727. The very simple construction that he adopted for this instrument (the plumb line itself being the index) did not admit of the use of a vernier: he therefore contented himself with dividing the arch of the limb of this instrument by primary points, as close as he thought necessary, that is, by divisions of five minutes each, and measuring the distance from the set of the instrument to the next point of division by a micrometer screw, in the construction of which screw he used uncommon care and delicacy. I have mentioned this instrument to introduce this observation; that I think it highly probable, had Mr. GRAHAM constructed the great quadrant *after* the zenith sector had been fully tried, he would have rejected not only the diagonals but the verniers also, as containing a source of error within themselves which may be avoided by a well-made screw*.

It seems also, that Mr. GRAHAM, at the time he constructed both these instruments, was not aware how much error could arise from the unequal expansions of different metals by heat or cold: for in both the radius, or frame of the instrument, was iron, while the limbs were of brass. They, however, remain in the Royal Observatory, perfect models, in all other respects, of every thing that is likely to be attained in their respective destinations, and monuments of the superlative abilities of that great mechanician Mr. GRAHAM†.

* This has been found consentaneous to the experience of my friend Mr. AUBERT, who, on my suggestion, has long since laid aside the use of his vernier, measuring always by the micrometer screws the distance between the set of the instrument, and the coincidence of the first stroke of the vernier with the next primary division of the limb.

† I have been informed, that Dr. MASKELYNE has caused this objection to the sector to be rectified, since its removal to the Royal Observatory, by substituting an iron limb instead of that of brass, the points being made upon studs of gold.

Mr. GRAHAM lived till the year 1751; and during his time there were few instruments of consequence constructed without his advice and opinion. They were for many years done by Mr. Sisson, to whom doubtless Mr. GRAHAM would fully communicate his method of division; and from this school arose that very eminent and accurate artist Mr. BIRD, whose delicate hand, joined with great care and assiduity, enabled him still further to promote this branch of division; and which being carried by him to a great pitch of perfection, the Commissioners of Longitude did themselves the credit, by an handsome reward, to induce him to publish to the world his particular method of dividing astronomical instruments; which being drawn up by himself, in the year 1767, this matter is fully set forth to the public: I shall therefore only take this opportunity of observing, that there seems to be one article in which Mr. BIRD's method may be still improved.

I must here observe, that I apprehend no quadrant, that has ever undergone a severe examination, has been found to form a perfect arch of 90° ; nor is it at all necessary it should: the perfect equality of the divisions throughout the whole is the first and primary consideration; as the proportion of error, when ascertained by proper observations, can be as easily and readily applied, when the whole error of the rectangle is fifteen seconds, as when it is but five.

In this view, from the radius taken, I would compute the chord of sixteen degrees only. If I had an excellent plain scale, I would use it; because I should expect the deviation from the right angle to be less than if taken from a scale of more moderate accuracy; but if not, the equality of the divisions would not be affected, though taken from any common diagonal scale. This chord, so prepared, I would lay off five times in succession, from

the

the primary point of 0 given, which would compleat eighty degrees; I would then bisect each of those arches of 16° , as prescribed by Mr. BIRD, and laying off one of them beyond the 80th, would give the 88th degree: proceeding then by bisection, till I came to an arch of two degrees, laying that off from the 88th degree, would give the point of ninety degrees. Proceeding still by bisection, till I had reduced the degrees into quarters = fifteen minutes each, I would there stop; as from experience I know, that when divisions are over close, the accuracy of them, even by bisection, cannot be so well attained as where they are moderately large. If a space of $\frac{1}{4}$ of an inch, which is a quarter of a degree, upon an eight-feet radius, is thought too large an interval to draw the index over by the micrometer screw, this may be shortened by placing another line at the distance of one-third of a division on each side of the index line, in which case the screw will never have to move the index plate more than one-third of a division, or five minutes; and the perfect equality of those side lines from the index line may be obtained, and adjusted to five minutes precisely, by putting each of the side lines upon a little plate, capable of adjustment to its true distance from the middle one, by an adjusting screw.

The above hint is not confined to the chord of sixteen degrees, which prohibits the subdivisions going lower than fifteen minutes: for if it be required to have divisions equivalent to five minutes upon the limb itself; then I would compute the chord of $21^{\circ} 20'$ only; and laying it off four times from the primary point, the last would mark out the division $85^{\circ} 20'$, pointed out by Mr. BIRD; *supplying the remainder* to a quadrant, from the bisected divisions as they arise, and not by the application of other computed chords.

In my Introduction to M. ROEMER's Method of Division, I have shewn, that divisions laid off in succession, by the same opening of the compasses, either in a right line, or in the arch of a circle, being in its idea geometrically true, and in itself the most simple of all processes, it has the fairest chance of being mechanically and practically exact, when cleared of the disturbing causes. The objection therefore to his method is, the great number of repetitions, which depending upon each other in succession (requiring no less than 540 to a quadrant, when subdivided to ten minutes each), the smallest error in each, repeated 540 times, without any thing to check it by the way, may arise to a very sensible and large amount: but in the method I have hinted, this objection will not lie; for, in the first case, the assumed opening is laid off but five times; and in the latter case but four times; nor does this *repetition* arise out of the nature of the thing; for, if you like it better, you may, in the former case, at once compute the chord of 64° ; and in the latter that of $85^\circ 20'$, and then proceed wholly by bisection; supplying what is wanted to make up the quadrant, from the bisected divisions, as they arise. Mr. BIRD prescribes this method himself, for the division of HADLEY's sextants and octants.

He, I suppose, was the first, who conceived the idea of laying off chords of arches, whose subdivisions should be come at by continual bisection; but why he mixed therewith divisions that were derived from a different origin (as prescribed in his method of dividing) I do not easily conceive. He says, that after he had proceeded by the bisections, from the arc of $85^\circ 20'$, the several points of 30° , 60° , 75° , and 90° , (all of which were laid down from the principle of the chord of 60° being equal to radius), fell in without sensible inequality; and so indeed they might; but yet it does not follow that they were equally true in their places.

as if they had been (like the rest) laid down from the bisection from $85^{\circ} 20'$, and therefore being the first made, whatever error was in them, would be communicated to all connected with them, or taking their departure from them. Every heterogeneous mixture should be avoided where equal divisions are required. It is not the same thing (as every good artist will see) whether you *twice* take a measure from a scale as *nearly* the same as you can, and lay them off separately; or lay off *two* openings of the compasses, in succession, *unaltered*; for though the same opening, carefully taken off from the same scale a second time, will doubtless fall into the points made by the first, without sensible error; yet as the sloping sides of the conical cavities made by the first point will conduct the points themselves to the center, there may be an error which, though insensible to the sight, would have been avoided by the *more simple process* of laying off the opening twice, without ever altering the compasses.

The 96 arc was, I have no doubt, invented by Mr. GRAHAM, from having perceived, in common with all preceding artists, how very much more easy a given line was to bisect, than to trisect, or quinquesect; and therefore the 96 arc which proceeded by bisections only (or by laying off the same identical openings, which, as already shewn, is still more simple and unexceptionable) was wholly intended by him by way of checking the division of the arc of 90 , which required trisections and quinquesections. But experience soon shewed the superior advantage of it so strongly, that the use of the 90 arc is now wholly set aside, where accuracy is required; whereas the ingenuity of Mr. BIRD having shewn a way to produce the 90 arc by bisection, when this is really pursued quite through the piece, by rejecting all divisions derived from any other origin, the 90 arc will have nothing in it to prevent its being
equally

equally unexceptionable with the 96 arc; and consequently if, instead of the 96 arc, another arc of 90 was laid down (which being upon a different radius, its divisions will stand totally unconnected with the former), then these two arcs would in *reality* be a check upon each other; for being of equal validity, the mean might be taken: and if (in lieu of vernier divisions) strokes at the distance of any odd number, as 7, 9, 11, or 13, are marked upon, and carried along with, the index plate; these will produce a check upon neighbouring divisions; and the angle may then be deduced from the medium of no less than four readings.

The last works that have been made known to the public in the line of graduation (so far as has come to my knowledge) are those of the very ingenious Mr. RAMSDEN, which were published, by order of the Board of Longitude, in the year 1777.

From his own information, I learn, that in the year 1760 he turned his thoughts towards making an engine for dividing mathematical instruments; and this he did in consequence of a reward offered by the Board of Longitude to Mr. BIRD, for publishing his method of graduating quadrants; for as, several years previous to that period, he had taken great pains to accomplish himself in the art of hand-dividing, in which line Mr. BIRD had acquired his eminence, he conceived by this publication of Mr. BIRD's he should be reduced to the same standard of performance with the rest of the trade. He, therefore, partly to save time, and that kind of weariness to an ingenious mind that ever must attend the endless repetition of the same thing from morning to night; partly still to preserve the pre-eminence he had then gained; and partly to procure dispatch in the great increase of demand for HADLEY's sextants and octants, in consequence of the successful application of the moon's motion to the

purpose of ascertaining the longitude at sea (which instruments for this purpose required a degree of accuracy and certainty in the division, by no means necessary thereto when applied to the simple purpose of observing latitudes); I say, for these considerations, Mr. RAMSDEN determined to set about something in the instrumental way, that should be sufficient effectually to answer these purposes.

Accordingly, considering the nature of the endless screw, he set himself to work upon an engine whose divided wheel or plate was of thirty inches diameter; and though the performance of this first essay was inferior to his expectations and wishes, yet with it he was able to divide theodolites with a degree of precision far superior to any thing of the kind that had been exhibited to the public.

This engine I myself saw in the spring of the year 1768; and it appeared to me not only a very laudable attempt towards instrumental divisions, but a very good model for the construction of an engine of the most accurate kind for that purpose. And he furthermore, at the same time, shewed me the model or pattern for casting a wheel of a much larger size, which he proposed to make upon the same plan, and with considerable improvements. This being effected some time in or about the year 1774, its accuracy was proved by making a sextant, afterwards subjected to the examination of Mr. BIRD; who in consequence approved the method, not only as fully sufficient for the division of HADLEY's sextants and octants for any purpose whatsoever, but in fact for dividing any instrument whose radius did not exceed that of the dividing wheel, which was forty-five inches in diameter: whereupon the Board of Longitude, ever ready to encourage all endeavours that tend to the certainty and perfection of any thing subservient to the purpose of finding the longitude at sea, very properly and usefully resolved to confer

an handsome reward on Mr. RAMSDEN, for delivering a full explanation of his method of making the said engine; which, in consequence, was published by order of the Board of Longitude in the year already mentioned, viz. 1777: the designs whereof are so full and explicit, that whoever could not understand that description, so as to enable him to make it, would be unfit to undertake it on other accounts.

From what I have said upon the works of the different artists that I have mentioned, it would seem that the art of graduation was brought to that degree of perfection, that nothing material can now be added thereto: and I should have been apt to have thought so myself, if I had not happened, in the course of my life, to have had a communication made to me (under the seal of secrecy) which seems to promise yet further light and assistance in perfecting that important art; and every impediment to the discovery thereof being now removed, I shall in the remainder of this essay give the clearest description thereof that I am able, with such elucidations and improvements as seem to be naturally pointed out by the method itself.

In the autumn of the year 1741, I was first introduced to the acquaintance of that then eminent artist, Mr. HENRY HINDLEY, of York, clockmaker;—he immediately entered with me into the greatest freedom of communication, which founded a friendship that lasted to his death, which did not happen till the year 1771, at the age of 70. On the first interview, he shewed me not only his general set of tools, but his *engine*, at that time furnished with a dividing plate, with a great variety of numbers for cutting the teeth of clock wheels, &c. and also, for more nice and curious purposes, furnished with a wheel of about thirteen inches diameter, very stout and strong, and cut into 360 teeth; to which was applied an endless screw, adapted thereto. The

threads of this screw were not formed upon a cylindric surface, but upon a solid whose sides were terminated by arches of circles. The whole length contained fifteen threads; and as every thread (on the side next the wheel) pointed towards the center thereof, the whole fifteen were in contact together; and had been so ground with the wheel, that, to my great astonishment, I found the screw would turn round with the utmost freedom, interlocked with the teeth of the wheel, and would draw the wheel round without any shake or sticking, or the least sensation of inequality.

How long this engine might have been made before this first interview, I cannot now exactly ascertain: I believe not more than about a couple of years; but this I well remember, that he then shewed me an instrument intended for astronomical purposes, which must have been produced from the engine, and which of itself must have taken some time in making*.

This

* This instrument was of the equatorial kind; the wheel parallel to the equator, the quadrant of latitude, and semi-circle of declination, being all furnished with screws containing fifteen threads each, framed and moved in the same manner as that of the engine; the whole of which instrument was already framed, and the telescope tube in its place, which was intended to be of the inverting refracting kind, and to be furnished with a micrometer. This, however, was not completed till some years after; but, in the year 1748, I received it in London for sale. It staid with me two years, in which time I shewed it to all my mechanical and philosophical friends, amongst whom was Mr. SHORT, who afterwards published in the Philosophical Transactions, vol. XLVI. N^o 493. p. 241. an Account of a portable Observatory, but without claiming any particular merit from the contrivance; however, the model of it differs from HINDLEY's equatorial only in the following articles. He added an azimuth circle and compass at the bottom. He omitted the endless screws, placing verniers in their stead; and at the top, a reflecting telescope instead of a refractor. This instrument of HINDLEY's being afterwards returned to him unaltered, I pointed out the principal deficiencies that I found therein; viz. that, in putting the instrument into different positions,

I in reality thought myself much indebted to Mr. HINDLEY for this communication: but though he shewed me his engine, and told me, that the screw was cut by the rotation of the point of a tool, carried round upon a strong arm, at the distance of the radius of the wheel from the center of motion, which arm was carried forward by the wheel itself, and the wheel was put forward by an endless screw, formed upon a cylinder to a proper size of thread, cut by his chock lathe; though he shewed me also this chock lathe, and the method employed to make the threads of the screw *equiangular* with the axis, that is, to free the screw from what workmen term *drunkenness*; and also shewed me how, by the single screw of his lathe, he could cut, by means of wheel-work, screws of every necessary degree of fineness * (and, by taking out a wheel, could cut a left-handed screw of the very same degree of fineness); by which means he was enabled not only to adapt his plain screw to the size of the teeth of his wheel, but also to prevent any drunkenness that otherwise the curved screw would be subject to in consequence of being produced from the plain

tions, the springing of the materials was such as in some positions to amount to considerable errors. This remained with him in the same state till the year of the first *Traité de Fluxus*, viz. 1761; when it was sold to ——— CONSTABLE, Esq. of Burton Constable, in *Haldensy*. Mr. HINDLEY, to remedy the evil above-mentioned, applied balances to the different movements. He soon afterwards completed one, *de novo*, upon this improved plan, for his Grace the late Duke of NORFOLK. A method of balancing in much the same way, without the knowledge that it had been done before, has been fully explained, and laid before the Society, by our ingenious and worthy brother, Mr. NAIRNE. Phil. Trans. vol. LXI. p. 108.

* A machine for cutting the endless screw of Mr. RAMSDEN's engine, upon principles exactly similar, is fully and accurately set forth in his Description of his dividing Engine above-mentioned.

one; furthermore, that the screw and wheel, being ground together as an optic glass to its tool, produced that degree of smoothness in its motion that I observed; and, lastly, that the wheel was cut from the dividing plate: yet, how the dividing plate was produced, he for particular reasons reserved to himself.

Nor can he be blamed for the reservation of this one secret; as he had, even at the time of my early acquaintance with him, a kind of foresight that from the superior merit of HADLEY's quadrant, a demand for that, and other instruments for the purpose of navigation, was likely to increase; and that he might live to see a public reward offered for a method of dividing them with greater accuracy and dispatch than had at that time appeared. Indeed, he had himself an idea, from the satisfactory success that had attended his operations in dividing, that a screw and wheel, produced from his engines of one foot diameter, would have as much truth as the eight-feet quadrant at Greenwich: and though he doubtless greatly over-rated the accuracy of these miniature performances, yet it does not follow, as his methods were not confined to so narrow a compass, but that, his scale of operation being proportionably enlarged, a degree of accuracy in the graduation of astronomical instruments may be attained in proportion.

I must here beg leave to observe, that there appears to me to be a natural limitation to the accuracy of instruments, consisting of considerable portions of a circle, such as quadrants, &c. *. I do not find that the finest stroke upon the limb of a quadrant, made by BIRD's own hand, if removed from its

* The zenith sector consists but of few degrees, with little variation of its position in using it.

coincidence with its index, can be replaced with any degree of certainty nearer than the 4000th part of an inch, though aided by a magnifying glass*.

A 4000th part of an inch being then determined to be the *minimum visibile* by the strokes of an instrument, this will be less than one second of a degree upon a radius of four feet; and therefore, if the whole set of divisions upon the limb could be preserved true to this aliquot part of an inch, the eight-foot quadrants of Greenwich might be expected to be true to half a second. How far they are from this, I do not exactly know; but I have reason to think they vary from it some seconds: nay, I believe it is generally allowed, that our largest quadrants, even when executed by the accurate hand of Mr. BIRD, do not exceed those of a less size, by the same hand, in proportion to their increase of radius: nor can it well be expected that they should; since, as the weight necessarily increases in a triplicate ratio of the radius, the great weight of the Greenwich quadrants in moving and fixing them (as they could not be divided in their place) may easily derange the framing; or even the *internal elasticity* of the materials may give way, by a change of position, to so minute a quantity as a 4000th part of an inch. It therefore appears to me, that since the divisions of a quadrant of four-feet radius are more than sufficient, and even those of three feet admit of all the distinctness that in other respects is wanted, a three-feet quadrant, in point of

* It will be to little purpose to attempt it with a greater power. Double microscopes can doubtless be formed to magnify objects, far less than a 4000th part of an inch, to distinct surfaces; but then the advantage of such degrees of magnifying power is chiefly upon the organized bodies of nature. Let a dot, or the finest point that can be made by human art, be so viewed, and it will appear not round, but a very ragged irregular figure.

size, is capable of all attainable exactness; and would be as much to be depended on as any of those now in being of eight feet. By adopting quadrants of this smaller size, we shall of course get rid of $\frac{1}{4}$ ths of the present weight; and consequently of much cumber, unhandiness, and derangement, that must arise from that weight, as well as the fear of totally discomposing them, if ever moved out of their place.

It now comes to be time to open a principle upon which there is a prospect of effecting such an improvement. I have shewn that a 4000th part of an inch is the *ultimatum* that we are to expect from *sight*; though aided by glasses, when observing the divisions of an instrument. But in the XLVIIIth volume of the Philosophical Transactions for the year 1754, I have shewn the mechanism of a new *pyrometer*, and experiments made therewith; whereby it appears, that, upon the principle of *contact*, a 24,000th part of an inch is a very definite quantity. I remembered very well that I did not then go to the extent of what I might have asserted, being willing to keep within the bounds of *credibility*: but on occasion of the present subject, I have re-examined this instrument, and find myself very well authorised to say, that a 60,000th part of an inch, with such an instrument, is a more definite and certain quantity than a 4000th part of an inch is to the *sight*, conditioned as above specified. The certainty of contact is, therefore, fifteen times greater than that of vision, when applied to the divisions of an instrument; and if this principle of certainty in contact did not take place even much beyond the limit I have now assigned, we never should have seen those exquisite mirrors for reflecting telescopes, that have already been produced.

These reflections apply immediately to my present subject, as HINDLEY'S method of division proceeds *wholly by contact*, and

that of the firmest kind; there being scarcely need of magnifying glasses in any part of the operation.

In the year 1748 I came to settle in London; and the first employment I met with was that of making philosophical instruments and apparatus. In this situation, my friend HINDLEY, from a principle the reverse of jealousy, fully communicated to me, by letter, his method of division; and though I was enjoined secrecy respecting others (for the reasons already mentioned), yet the communication was expressly made with an intention that I might apply it to my own purposes.

The following are extracts from two letters, which contain the whole of what related to this subject; and since I have many things to observe thereon, so that the paraphrase would be much greater than the text, I think it best not to interrupt the description with any commentary, as perhaps his own mode of expression will more briefly and happily convey the general idea of the work than any I can use instead of it.

MY DEAR FRIEND,

York, 14 Nov. 1748.

AS to what you was mentioning about my brother's knowing how I divided my engine plate, I will describe it as well as I can myself; but you will want a good many things to go through with it.

The manner is this: first chuse the largest number you want, and then chuse a long plate of thin brass; mine was about one inch in breadth, and eight feet in length, which I bent like an hoop for an hog's head, and folded the ends together; and turned it of equal thickness, upon a block of smooth-grained wood, upon my great lathe in the air (that is, upon the end of the mandrel): one side of the hoop must be rather wider than

the other, that it may fit the better to the block, which will be a short piece of a cone of a large diameter: when the hoop was turned, I took it off, cut, and opened it straight again.



The next step was to have a piece of steel bended into the form as *per margin* *; which had two small holes bored in it, of equal bigness, one to receive a small pin, and the other a drill of equal size. I ground the holes after they were hardened, to make them round and smooth. The chaps formed by this steel plate were as near together as just to let the long plate through. Being open at one end, the chaps so formed would spring a little, and would press the long plate close, by setting in the *vise*. Then I put the long plate to a right angle to the length of the steel chaps, and bored one hole through the long plate, into which I put the small pin; then bored through the other hole; and by moving the steel chaps a hole forward, and putting in the pin in the last hole, I proceeded till I had divided the whole length of the plate.

The next thing was to make this into a circle again. After the plate was cut off at the end of the intended number, I then proceeded to join the ends, which I did thus: I bored two narrow short brass plates † as I did the long one, and put one on the inside, and the other on the outside of the hoop, whose ends were brought together; and put two or three turned screw pins, with flat heads and nuts to them, into each end, which held them together till I rivetted two little plates, one on each side of the narrow plate, on the outside of the hoop. Then I took out the screws, and turned my block down, till the hoop

* The figure is considerably less than the real tool should be.

† These I shall hereafter distinguish by the name of *saddle-plates*.

would fit close on ; and by that means my right line was made into an equal divided circle of what number I pleased.

The engine plate was fixed on the face of the block, with a steel hole fixed before it, to bore through ; and I had a point that would fall into the holes of the divided hoop ; so by cutting shorter, and turning the block less, I got all the numbers on my plate.

I need not tell you, that you get as many prime numbers as you please ; nor that the distance of the holes in the steel chaps must be proportioned to the length of the hoop.

You may ask my brother what he knows about my method of dividing ; but need not tell him what I have said about it ; for I think neither *he* nor *John Smith* know so much as I have *told you*, though I believe they got some knowledge of it in general terms *.—I desire you to keep the method of dividing to *yourself*, and conclude with my best wishes,

and am, dear Sir, yours, &c.

HEN. HINDLEY.

Though the above letter was in itself very clear and explicit, as to the general traces of the method, yet some doubts occurring to me, a further explanation became necessary. A copy of my letter not being preserved, the purport of it may be inferred from the answer, which was as follows :

* The persons here referred to were both bred with him. His brother, Mr. ROGER HINDLEY, who has many years followed the ingenious profession of a watch-cap-maker in London, was so much younger as to be an apprentice to him. Mr. JOHN SMITH, now dead, had some years past the honour to work in the instrument way, under the direction of the late Dr. DEMAINBRAY, for his present MAJESTY.

DEAR FRIEND,

York, 13 March, 1748-9.

I THINK in your last you seem to be apprehensive of some difficulties in drilling the hoop for dividing: First, that the center of the hole in the hoop might not be precisely in the center of the hole of the steel chaps, it was drilled in; but if I described fully to you the method I used, I can see no danger of error there: for my chaps were very thick, and the two corresponding holes were a little conical, and ground with a steel pin; first one pair, and then the other, alternately, till the pin would go the same depth into each. Then for drilling the hoop, I took any common drill that would pass through, and bore the hole. After that I took a five-sided broach, which opened the hole in the brass betwixt the steel chaps, but would not touch the steel; so consequently the center of the holes in the brass must be concentric with the holes in the chaps: and for alterations by air, heat, cold, &c. I was not above two or three hours in drilling a row of holes, as far as I remember.

2dly, For drilling, in a right line, I had a thin brass plate, fastened between the steel chaps, for the edge of the hoop to bear against, whilst I thrust it forward from hole to hole. What you propose of an iron frame with a lead outside, will be better than my wooden block; but considering the little time that past, betwixt transferring the divisions of the hoop to the divisions of my dividing plate, I did not suffer much that way. It was when I drilled the holes in my dividing plate that I used a frame for drilling, which had one part of it that had a steel hole, that in lying upon the plane of the dividing plate was fixed fast in its place for the point of the drill to pass through: then, at the length of the drill, there was another piece of

steel, with a hole in it, to receive the other end of the drill to keep it at right-angles to the plane of the plate. This piece was a spring, which bended at the end, where it was fastened to the frame of the lathe, at about eighteen inches from the end of the drill; so it pushed the drill through with any given force the drill would bear: and though that end of the drill moved in the arch of a circle, it was a very small part of it, being no more than equal to the thickness of the dividing plate.

My good wishes conclude me yours,

HEN. HINDLEY.

Whoever attentively considers the communication contained in the above letters will see, that more happy expedients could not have been devised to procure a set of divisions, where there should be the most exact equality among *neighbours*: and which, for the purposes of clock-making, is the principal thing to be wished for. But herein, as in M. ROEMER's method, there were no means of checking the distant divisions, which run on to 360: now such a check, when the expansion of metals is considered, and particularly the difference of expansion between brass and steel, seems absolutely necessary for the purpose of divisions upon instruments, where the accurate mensuration of large angles is required, as much as the equality of neighbouring divisions*.

With this view the invention of this ingenious person suggested to him the thought of making his curved screw to lay

* The ingenious Mr. STANLIFFE (some years a workman of HINDLEY's) has suggested, that the difference of expansion between the steel chaps and the brass hoop may be avoided by making the chaps of brass also, with hard steel holes set separately therein, somewhat similar to the jewelled holes of watches.

hold of fifteen teeth or degrees together: this, in effect, becomes a pair of compasses, 24 removes of which complete the whole circle, and produce 24 checks in the circumference: and whoever considers the very exquisite degree of truth that results from the grinding of surfaces in contact, as already noticed, must expect a very great degree of rectification of whatever errors might subsist in the wheel after its first cutting.

What degree of truth it might in reality be capable of upon its first production and adjustment, is not now to be ascertained, he never having used it for the graduation of any capital instrument. Those that he made with a view to an accurate measure of angles, he always made with a screw and wheel, or parts of circles cut by his engine into teeth, and ground together as before-mentioned; but I have reason to think, that its performance, if put to a strict test, was never capable of that accuracy that he himself supposed it to have.

The method itself, however, from its simplicity, and ease of execution, seems to me to be a foundation for every thing that can be expected in truth of graduation; and in consequence for reducing instruments to the least size that is capable of bringing out all that can be expected from the largest; when it shall, like manual division, have received those advantages that the joint labours of the most ingenious men can bestow upon it. That I may not appear to be without grounds for my expectations, I will beg leave to propose, what near forty years occasional contemplation has suggested to me on the subject; and as I can describe the process I would pursue, where different from HINDLEY'S, in fewer words than I could make out a regular criticism upon his letters, I will immediately proceed to the description of it.

Proposed Improvements of HINDLEY's Method.

I would recommend the number of parts into which the circle is to be reduced to be 1440; that is 4 times 360; which divisions will therefore be quarters of a degree; the distances of the holes in the chaps will therefore, to a three-foot radius, be $\frac{1}{16}$ of an inch nearly; that is, between the one-sixth and one-seventh of an inch distance center and center.

Having provided myself with a stout mandrel, or arbor, for a *chock Lathe*, properly framed, that would turn a circle of six feet diameter, I would prepare a chock, or platform, for the end of it, of that diameter, or a little more, composed of clean-grained mahogany plank, all cut out of the same log; which, when finished, to be about $1\frac{1}{2}$ inch thick, and formed in sectors of circles, suppose 16 to make the circle; the middle line of each sector lying in the direction of the grain of the wood, this will consequently every where point outward: the method of framing this kind of work is well known.

The way of getting a slip of brass to answer the circumference of this platform is suggested in Mr. BIRD's Account of constructing Mural Quadrants. Let a parallelogram of brass of about three feet long, and of a competent substance (suppose half an inch) to make it when finished about one-twentieth of an inch in thickness, be cast of the finest brass; and this to be rolled down till it becomes of sufficient length for the hoop, and about one-fifth part more. I would then cut off, from the whole length, somewhat better than one-sixth part, the whole being sufficiently reduced to a thickness by the rollers. Perhaps no way will be more ready and convenient to

get

get such a long strip of brass reduced to an equal breadth, than the method prescribed by HINDLEY; viz. by turning it upon the chock prepared; but I would not make it wider on one side than the other, like the hoop of a cask, as he describes, but exactly to fit the chock, when truly cylindric; for the internal elasticity of the brass, in so great a length, will be very sufficient for fitting it on tight enough, without any tapering. This I will now suppose done; and a pair of steel *chaps*, as described by HINDLEY, to be also prepared, and ready for grinding; which, by such a careful admeasurement as can easily be made, will give the length of the hoop sufficiently near, on its first preparation.

Method of forming a Pair of Straps as a check to the Divisions.

The part first cut off must be again cut into two equal parts in length; which, for distinction sake, I will call the *straps*; and which are to serve as checks to every 60th and every 120th division of the circle.

A steel plate, of about half an inch in breadth, the same thickness as the straps, and in length equal to the breadth of the hoop plate, must be foldered with silver solder to one end of each of the straps, by which means their length will be increased half an inch by the steel. An hole must then be made through each steel plate, of the same size as those through the chaps, and answerable to the middle of the straps; but so near the border of the steel, that when the chaps are put on, and adapted to the steel hole, the next hole will fall through the brass. The steel plates must then be hardened; and a pin being put through the two holes and the two plates, these must be

be wrought to a right line in contiguity to each other; by this means the straight edge of each of the straps will be reduced to the same distance from the steel hole: the hard steel edges may be rectified by the grindstone, if necessary.

This being done, not only the holes in the chaps, but the holes in the two steel plates, applied to each other, like the two sides of the chaps, must be respectively ground together; not with a taper pin, as prescribed by HINDLEY; but so as not only to be cylindrical, but that the same cylindrical pin shall equally fit them all, and leave them smooth and polished; which is a process no ways difficult to a curious artist, and of which therefore a minute description is unnecessary.

The chaps being then put upon one of the straps, with its straight edge uppermost, and a pin put through the holes on the left-hand, and through the steel hole in the strap under operation, the chaps must be set upright, so that the line joining the centers of the holes shall be parallel to the upper edge of the strap; the brass plate, mentioned by HINDLEY, between the chaps, as a guide for directing them always to that upright position, may be then adjusted and fixed to the inside of the chap next the operator*.

The performance of the ensuing part of this work should be at a season when the temper of the air is not very variable; rather above the mean temper (suppose at 60°) than below it;

* It would be well, previous to the drilling of the steel chaps, that another hole was drilled in the chaps, that should be somewhat above the upper edge of the straps, and in the middle betwixt side and side, to receive a *steady pin* therein, antecedent to drilling the main holes; for then a tempered steel pin, a little taper, will, by driving it in as far as necessary, constantly answer this purpose from first to last, so as to regulate the holes in grinding, to be truly opposite: proper holes should also be drilled for fixing the brass guide plate to one of the chaps.

but above all things the artist should be himself cool ; that is, not in a state of sensible perspiration ; and there should be a free circulation of air in the room. Things being thus conditioned in respect to temperature, he may begin to drill the holes in one of the straps ; the pin being first put through the chaps and through the steel hole of the strap ; and the next hole, being drilled through the brass with a common drill, that and every hole as it goes is to be finished with a taper broach, as prescribed by HINDLEY ; and he may then prove or finish every hole by the application of a thorough broach, made so full as to require a degree of pressure to force it through ; and this broach being a little tempered, and the holes quite hard, there will be no fear of injuring the steel holes*.

Calling the hole in the steel plates o, and observing the time of beginning, you may proceed to drill 60 holes as prescribed by HINDLEY ; and noting how long you have been about it, you may lay the work aside a length of time, equal to the time you took in drilling ; that any addition of warmth it may have acquired in handling or working may be again lost in a great degree †. After this pause you may begin again, and go on to finish 60 holes more ; that is, to the length of

* The steel holes in the chaps need not to be above one-twentieth of an inch in diameter ; and though it may be proper to make the steel plate, of which they are formed, one-tenth of an inch thick, in order to give the spring formed between them a convenient degree of stiffness, yet they may be reduced (by chamfering the outsides) to half that thickness.

† As there is not much occasion for the artist to touch his work, the effects of that may also be very much avoided by wearing thick gloves ; and the friction being but slight, and the work almost continually in the vise, the variation of temperature in the metals concerned cannot be sensible or considerable.

120 holes from the beginning; you then proceed in the same manner with the other strap.

Method of drilling the Hoop.

You are now prepared to commence the work upon the long or *hoop-plate*; and you proceed therewith, in forming the first hole with the chaps, as before directed by HINDLEY, and this first hole you call 0. You then place the straps one on each side the hoop, with their gaged edges upward, and put the pin through the holes denominated 60 upon the straps, and through the first hole already made, and denominated 0 upon the hoop; then, bringing the gaged edges of the steel plates to be even with the upper or working side of the hoop, you pinch them together in the vise, and drill and broach the hole through the steel plates, which will make the hole, number 60, upon the hoop. This done, you put the pin through the left-hand hole of the chaps, and the hole marked 0 upon the hoop-plate first made, and proceed to drill with the chaps to 59 holes inclusive, which will fill up the whole space from 0 to the 60th division before obtained.

You now again have recourse to the straps, and placing them one on each side the hoop-plate, you put the pin through the 120th hole of the straps, and through the hole marked 0 upon the hoop-plate; and regulating the steel plates to the hoop-plate as before, you drill and form a hole with the steel plates, which will correspond with the 120th hole upon the hoop-plate; and afterwards filling up the 59 holes wanting, by means of the chaps, you then have all completed to the 120th division, which is one-twelfth of the whole circle.

You then proceed, in like manner, with another set of 120 holes; that is, placing the 60th hole of the straps to the 120th hole of the hoop-plate, and from it producing the 180th hole; you, in like manner as before, fill up this 60 with the chaps; and afterwards placing the 120th hole on the straps in the 120th hole on the hoop-plate, you will obtain the 240th hole; so that filling up this last set of 60 divisions, you have obtained 241 holes, including 240 spaces or divisions of the hoop; and repeating this process ten times more, you will, in like manner, obtain 1441 holes, comprehending 1440 spaces*. And this process being carried on in temperate weather, the manner of working produces twelve similar operations, wherein the materials and tools concerned will not only be subject to very little change of temperature, but that change, whatever it is, will be nearly similar in each set of 120 holes: we may therefore infer, that the greatest inequality, or indeed any that can be sensible, must be at every 60 divisions, that is, between the 50th and 60th, and between the 110th and 120th, both which will be equally repeated 12 times, in the whole length which is to compose the *circumference of a Circle*, and which will thus be checked thereby 12 times in the circumference, and 12 times more at the intermediate distances; that is, with 12 master checks, and 12 subordinate ones, in the whole round.

It is proper here to observe, that in M. ROEMER'S method even sixty divisions could scarcely be trusted in an affair of great accuracy, on account of the objections already made, arising from the points having such slight hold in the surface of the brass; but here the parts are held so exceedingly firm, and the

* It will be proper, for reasons hereafter to be mentioned, to continue the divisions to 20 holes more, making in the whole 1461 holes.

operation carried on with so much power, that any small inequality in the hardness of the brass, or irregularity of surface, cannot be supposed to affect the place of the center of the hole; nor will any small inequality that may be suspected from the wear of the steel holes sensibly affect the center of the hole, to which every thing is ultimately referred.

Method of joining the Hoop.

A more happy thought than that of HINDLEY's, for joining the two ends of the hoop, could scarcely have been wished for, in regard to preserving the same equality of the space between the holes contiguous to the joint, as in the other parts: for though, geometrically speaking, the two *saddle* plates, in which the little cylindrical bolts are fixed, for bringing the terminating holes of the hoop plate to their due distance, being one applied within the hoop, and the other without, will belong to circles of different *radii*; yet this difference being exceedingly small in such thin metal, and so great a radius, and one being as much too big for the hoop as the other is too little, when the bolts are put in, and the hoop in that part set nearly to a circle by a mould; the mean between them assumed by the hoop, from the elastic compressibility of the materials, will be the truth.

It must, however, be remarked, that in the use of the straps, the joining of the hoop should not be made at any part betwixt an 119th and an 120th division, as some inequality must be supposed there, unless the saddle plates were adapted thereto. The method the most easily practised, will be to continue the division upon the hoop, about twenty more than the completion of the number intended to form the circle, and to cut off all the overplus ones at the beginning.

The

The saddle plates I would recommend to contain ten holes each ; so that if the divisions are carried on to twenty more than what will be contained in the circle, there will be a piece containing twenty to cut off ; and this again being cut in the middle will afford ten holes to make each saddle plate ; so that there will be a place for a bolt on each side the joint, and then putting a bolt through every other hole, there will be three bolts at an end.

The pieces destined for the saddle plates, thus obtained, being broader than can be admitted when put to this use, I would advise to divide the breadth of the plate into three equal parts ; and with a cutting hook (which perhaps will be attended with the least violence in the separation) to separate the two outside pieces from the middle piece : by this means the two saddle plates (though double) will occupy one third only of the breadth of the hoop in the middle ; and two of the pieces cut off being applied, one on each side of the saddle plate on the outside, will answer in like manner for the *rivet* plates.

The last operation to compleat the joining of the hoop is the putting on the rivet plates : to compleat this, I would advise a piece of brass, of three or four inches in length, to be filed so as to answer to the inside of the hoop, when reduced to a true circular form ; and being three-eighths, or one-half an inch in thickness, to file the opposite side somewhat nearly concentric thereto ; apply the middle of its convex arch to the inside of the hoop at the joint, and then bringing on the middle of one of the rivet plates to the joint of the hoop, confine the three together by a couple of narrow-chapped hand vices, leaving a space between them capable of receiving a couple of pins as rivets on each side the joint ; the holes for the rivets are then to be drilled through all, and a little smoothed with a broach at their
their

their entry, into which smooth taper pins are to be driven; not with violence, but moderately, that no sensible stretching of the solid parts may take place thereby; then cutting off and smoothing the heads, shift the vises so as to receive another couple of holes, and a third couple in the same end of the hoop; and proceed progressively in the same manner, from the middle to the other end of the rivet plate; then gently separate the internal brass mould with a thin knife, or such like instrument; and cutting off, and very lightly rivetting the inner ends, proceed to fix the other rivet plate, in the same manner, on the other side: by this means the hoop will be firmly joined in the very position given it by the saddle plates and mould. These plates may then be removed, the inside of the hoop cleared and smoothed, if necessary; and the outside will have the middle part clear where the divisions lie, and that without sensible loss or gain in the juncture.

Method of transferring the Divisions of the Hoop to a dividing Plate.

The hoop being thus refitted for the chock, that should be turned down to leave a shoulder on one side, that the hoop, now reduced to an equal breadth, may be forced against it; and the divisions, being equally distant from one of its edges, will be all found in a circle, as if turned upon it. It should be very carefully fitted to the chock, that it may go on with a sufficient degree of tightness, and without the necessity of much forcing; and it will be no inconvenience now, if it goes on upon a very slight degree of taper of the chock, as the internal spring of the materials will easily accommodate it to this shape without any injury to its general truth: a slight degree of a groove should

be:

be turned in the place where the divisions will come, that any conical pin, that is to serve as an index, let drop into the divisions or holes, may not, by reaching through this thin plate, abut upon the wood, rather than upon the sides of the holes: and thus this hoop is made into a wheel of 1440 equal divisions, moveable round upon its own axis, whereon it was formed.

Against the time that this is completed, there must be prepared a flat circular plate or wheel of brass, the rim of which should be of about $3\frac{1}{2}$ inches breadth, and about two-tenths of an inch in thickness; when finished, to make a dividing plate; the external diameter of this is to be such, that when laid flat upon the surface of the mahogany platform, its extreme edge will exceed the diameter of the hoop by about half an inch all round. There must also be prepared brass arms (suppose eight in number) of an equal substance with the outer rim, and all connected with a circular plate in the middle; and, the whole of this work being framed beforehand, is to be let on flat upon the mahogany platform; whose face is supposed to be turned truly flat, and sufficiently affixed with screws: in this situation, the outward edge is to be turned, and the outward face of the rim turned flat. The *center plate*, which may be about twelve inches diameter, is also to be turned as flat as possible, and a center hole, of about half an inch diameter, to be very carefully turned therein.

A piece of clean, straight-grained, well seasoned mahogany, of about two feet long, three inches thick, and five or six inches broad, is then to be well affixed to some part of the general frame of the lathe, which must now have its position altered, so that the platform will become horizontal; and therefore the frame should

be

be originally made with this view *. The piece of mahogany is to be affixed so that one of its larger faces shall be in a parallel plane to the face of the platform, and so low as to clear the under side of the platform in its rotation; and so far distant from the center, that an index may be fixed upon this upper face of the piece of wood, so as conveniently to drop into the holes of the hoop; while the common *cutter frame* of a clock-maker's engine shall be firmly attached upon the same face of the wood, and so fixed as to cut the edge of the dividing plate into teeth, answerable to the several divisions of the hoop. The teeth need only to be cut with a common cutter, making a parallel notch: and here it will be proper to observe, that not only both the index and cutter are to be founded on the same piece or base of wood; but that the nearer they are together, the more free they will be from the effects of all variations of expansions by variations of temperature †.

The equalising the Teeth of the dividing Plate by grinding.

The object of transferring the divisions of the hoop to the teeth of the dividing plate, is still farther to equalise the teeth by grinding; especially those that, falling within the compass of

* After changing the position of the lathe, the collar of its mandrel should be removed, and the neck made to move within three planes, so as to preserve an exact center, in the manner of an *equal altitude* instrument.

† It is proper to observe, that as it may be impracticable to get the rim of the dividing plate cast of the proper size, in one entire piece, it will be very practicable, if cast of a less size (suppose half), but of a sufficient thickness, to roll it down; and by having the outward edge originally thicker than the inner, in the proportion of the *radius*, it may be so managed by the rollers as to be of an equal thickness when brought to its proper size. But the arms and center plate should be of the same metal, rolled in the same degree.

each set of 120 divisions, may be supposed, if any, to be mended thereby; but as it may be incommodious to construct a curved screw, of such a length and size, in HINDLEY's method, as would be sufficient for the purpose, I would propose to use two screws of brass, cut from a cylinder in the way set forth by Mr. RAMSDEN, each of which, with a very little grinding upon this large circumference, would lay hold of ten or twelve teeth together. I would place the two screws, that is, their middles, to be ninety divisions asunder; of consequence, when one of the screws is between the 59th and the 60th, or between the 119th and 120th division of each set, the other will be in the middle of the space divided by the chaps only*.

The threads of these screws I would advise to be cut a little taper, so that as they grind in, they may fill the notches of the teeth; which also, by this means, will acquire a little tapering towards their extremities; and by cutting the notches parallel, as I have mentioned, the true ground part will always be certain of being at the extremity.

When the screws have been used in grinding till they are found to have the effect of a perfectly equal and easy rotation all round, and all the teeth reduced to a sensible taper, and regular bearing, I would then totally remove the screws from the square block of wood, upon whose upper face I suppose them to have been mounted; in like manner as I suppose the index and cutting frame to have been removed, to make room for the mounting of

* The best way of giving an equal motion to those two screws, seems to be by a detached axis, carrying two common flat wheels; one acting upon a like flat wheel, upon the axis of one screw, and the other, in the same manner, upon the other; and applying the pulley for communicating the power to the middle of the detached axis between the two wheels, the spring or twist will be equal both ways; so that in turning the contrary way round, they will still be in equal advance.

the screws. I now consider the teeth of the dividing plate, so formed, as having all the equality that the present known state of human art has pointed out; and the whole convertible upon the axis or mandrel upon which it has been originally formed, and the central hole of the plate concentric therewith; I therefore consider the ground faces of the teeth of the plate as the actual divisions. It now remains to shew how they are to be transferred, to form the divisions of an instrument.

Preparation of the dividing Plate for graduating Instruments.

If a small cylinder of hard steel is duly polished, and made of a size so as just to chock in betwixt the extremities of the teeth, then the center of that cylinder will be a fixed point, in respect to the circumference of the wheel: if another cylinder is applied in like manner, at the distance of a number of divisions (suppose it a prime number, so as to cross all former divisions, viz. 17 or 19), then the middle of the line joining the centers of the two cylinders will remain in the direction of the *same radius*, though one of them should force in a minute quantity further than the other; and if a point is assumed in the direction of a tangent to a circle at this middle-point, then though both the cylinders should drop in a minute quantity further at one time than another, yet the middle-point would remain at the same distance from the point in the tangent; provided that point was removed to a competent distance, that is, to five or six inches. On this principle I would construct an index, the two cylinders being fixed in a frame, convertible about the middle-point, and to be centered in the end of the lever, representing the tangent; then this lever being again convertible about the point in the tangent line, the middle-point would always have a fixed distance

from the point in the tangent, and there hold it steadily fast; the tangent point being placed upon the fixed block before-mentioned.

Use of the dividing Plate in the Graduation of Instruments.

Our dividing plate is now ready for the reception of an instrument; suppose it a quadrant, whose radius, however, must not exceed the radius of the dividing plate: It is to be laid upon the face of the dividing plate, and a weight, or weights, equivalent to that of the quadrant, is placed on the opposite side, to balance it. It must also be supposed, that the quadrant is made with a view to be divided by this engine; and consequently, that the central cylinder is so well adapted, and nicely fitted to the center hole of the quadrant, that the center cylinder can be removed, in order for the limb to be divided, and again replaced, without sensibly altering its center. This being the case, let a piece of metal be turned, to apply to the quadrant, perfectly like its center cylinder at the upper end; and turned nicely to fit the central hole in the dividing plate, at the lower end; then, the quadrant being fixed with proper fastening screws, I would cut the divisions with a beam compass; and, if a fixed point is assumed, *viz.* the center of the tangent point for the index; then the beam compass being always opened to the computed length of the tangent of the circle of divisions, it will be sufficiently near for cutting the divisions, square to the circular arches between which they are placed.

It will also be proper (to prevent unequal expansions) that the beam of the compass should be formed of a piece of clean-grained *white fir*; and that the length between the points be inclosed in a tube of tin or brass; without touching the beam,
except

except at the terminations, which will in a great measure protect it from both alteration of moisture, and of heat from the body of the artist, during the operation.

It will be likewise proper to have a lever, or some equivalent contrivance, to bring the dividing plate forward; that after lifting the little cylinders out of the divisions, and resting them upon the tops of the teeth, they may be brought gently forward with an equal drag, and ultimately snap in between the teeth, by the strength of the spring commanding the index; by this means the drag of the friction of the whole will be constantly the same way.

Conclusion.

Now, if, as it has been shewn, a quadrant of any radius may be read off to the 4000th part of an inch, then this quantity upon a radius of three feet will not be so much as $1\frac{1}{2}$ second; and as the whole of the process is carried on by contact, in which a greater error than that of a 60,000th part of an inch cannot be admitted in any single operation, I should assuredly expect a three-foot quadrant, so divided, to be true in its divisions, and read off to at most two seconds.

But, after all, in an instrument like this, I should expect the greatest source of error to be in the want of perfect coincidence of the center of the divisions with the actual center upon which the index revolves; and therefore, that if, instead of a quadrant of three-feet radius, a complete circle of five feet diameter was divided, and its divisions read off from the two opposite points (taking the mean), then the errors of the center will be wholly avoided. For this reason, I am very clearly of opinion, that the sagacious proposition of Mr. RAMS-

DEN,

DEN, to use circles instead of quadrants, or other portions of circles, will bid much the fairest for perfection in actual practice; and that his ingenious method of making them both stiff and light, by the use of hollow conical tubes by way of spokes, in the manner of a common wheel, will enable him to mount them of five feet diameter, upon hollow axes, in the nature of a *transit*. By this means we shall have all the good properties of both the quadrant and transit united in one instrument; and observations both of right ascension and declination, through the very same telescope, as long since attempted by M. ROEMER; and to a degree of perfection and certainty, in point of declination, hitherto unattainable by the largest instruments that have yet been made.

N. B. In matters of very nice determination, small circumstances often come to be of consequence; and it is in this view that I mention what follows. It was a practice of HINDLEY's of many years standing, and since followed by myself and others, wherever he made any use of the *vernier*, to lay the vernier plate in the same plane, or cylindrical surface continued, whereon the principal divisions are cut. It is of equal utility, though the vernier be rejected, to lay the index stroke in the plane of the divisions. In this way the divisions being by convenience upon the external border of the limb*, *trapp* sets of divisions are thereby rendered incommodious; but those

* It has been objected, that laying the divisions upon the extreme edge of the limb of the instrument subjects it to injury: but, to obviate this, in an HADLEY's quadrant made for me, by my direction, by the late Mr. MORGAN, in the year 1756, wherein the vernier is laid even with the divisions, those are protected by a projection of the solid part of the limb, beyond the divisions; a *Rabbet* being sunk in the edge of the limb, to clear the vernier.

that with two sets, as a check, will in a great measure aid themselves, by reading from two different parts of the same set of divisions; which is very easily provided for, by putting an additional stroke upon the index plate, at the distance of 9, 11, or any prime number of divisions to 19, 23, or more; and reading off from that stroke also; as before recommended for great quadrants, where the vernier is proposed to be rejected*: so that they will thereby be mutually checked by divisions that had no correspondence in their original formation.

* I would not have it thought, from my proposal of rejecting the vernier, that I have any quarrel with it; I think it a very simple and ingenious contrivance, where it is properly applicable; that is, where the strokes of the vernier, or their estimated halves, are sufficient for all the precision required or expected from the instrument, as in HADLEY's quadrants, theodolites, &c.: but where still more minute divisions are required than can easily be had by estimation from the vernier; to do this by a screw, as a *supplement* to the vernier, appears to me in the light of bringing a more accurate tool to supply the deficiencies of one less accurate; when the former might, with more propriety, supply the place of the latter altogether.



II. *A Series of Observations on, and a Discovery of, the Period of the Variation of the Light of the Star marked δ by Bayer, near the Head of Cepheus. In a Letter from John Goodricke, Esq. to Nevil Maskelyne, D. D. F. R. S. and Astronomer Royal.*

Read November 24, 1785.

S I R,

York, June 28, 1785.

THE improvements which of late years have been introduced into astronomy, should be attributed not only to the diligence and accuracy wherewith astronomers prosecute their observations and discoveries, but in part also to your exertions, and especially to that kind encouragement which you have, on many occasions, afforded those who make this science their chief study; and I am happy to have this opportunity of acknowledging myself one of those who are much indebted to you in this respect. Under these impressions I thought I could not do better than to address to you the following account of a periodical variation in the star δ Cephei, which I lately discovered. This account will, I presume, be a considerable addition to the few discoveries that have but very lately been made respecting the same subject. They may probably lead to some better knowledge of the fixed stars, especially of their constitution and the cause of their remarkable changes.

My

My first observation was Oct. 19, 1784; and as I wished to establish the several points of the variation with as great accuracy as the nature of the subject will admit of, I have delayed sending this account till now; but as observations made through so long an interval of time must be very numerous, and would only swell this paper to an unnecessary length, I have in the following series formed a selection, chiefly of those that were made under the most favourable circumstances; and I must add, that none of those that are omitted contradict the results. From this series I have settled, that the star has a periodical variation of 5 d. 8 h. 37 $\frac{1}{4}$, during which time it undergoes the following changes:

1. It is at its greatest brightness about one day and thirteen hours.
2. Its diminution is performed in about one day and eighteen hours.
3. It is at its greatest obscuration about one day and twelve hours.
4. It increases in about thirteen hours.

When it is in the first point it appears as a star of between the fourth and third magnitude; but its relative brightness does not seem always to be quite the same, being sometimes between ζ and ι Cephei, and sometimes only equal to, or something less than, ι Cephei, or between ζ Cephei and γ Lacertæ. In the third point it appears as a star of between the fourth and fifth magnitude, if not nearer the fifth; and its relative brightness is as follows: nearly equal to ι and ξ Cephei, and considerably less than γ Lacertæ.

The relative brightness and magnitude of those stars to which the variable one was compared, is as follows: ζ Cephei, the brightest, is between the third and fourth magnitude; ι Cephei,

the next brightest, is between the fourth and third; γ Lacertæ is less than ι Cephei, and of about the fourth magnitude; ϵ Cephei is between the fourth and fifth magnitude; and ξ Cephei, which is a little less than ϵ , is between the fifth and fourth.

A Series of Observations on the Variation of the Light of the Star δ Cephei.

1784, Oct. 19, at 8 $\frac{1}{2}$ h. I thought it was rather less than ζ Cephei.

Oct. 20, at 8 $\frac{1}{2}$ h. it was rather less than ζ , and about equal to ι Cephei.

Oct. 22, at 9 $\frac{1}{2}$ h. less than ι , and larger than ϵ Cephei; but the air was not very favourable.

Oct. 23, at 6 $\frac{1}{2}$ h. and 11 h. less than γ Lacertæ, and a little brighter than ϵ Cephei.

Oct. 24, at 6 $\frac{1}{2}$ h. less than ζ Cephei, somewhat less than ι Cephei, and something brighter than γ Lacertæ; strong moon-light, and air rather hazy.

At 8 $\frac{1}{2}$ h. to 11 h. a little less than ζ and ι Cephei, and brighter than γ Lacertæ; air clear and frosty; the moon was very low at 11 h.

Oct. 25, at 6 h. 8 h. and 11 h. nearly the same; air pretty clear, and moon bright.

Oct. 26, at 9 $\frac{1}{2}$ h. and 11 h. rather less than γ Lacertæ; strong moon-light, but air very clear.

Oct. 27, 6 $\frac{1}{2}$ h. and 10 $\frac{1}{2}$ h. less than γ Lacertæ, and brighter than ϵ Cephei; ditto.

Oct. 28, at 9½ h. and 12 h. just the same, if not less; moon-light, but the air was remarkably clear.

Oct. 31, at 8 h. nearly equal to, if not less than, 7 Lacertæ.

Nov. 1, at 11½ h. somewhat less than 7 Lacertæ; air clear.

Nov. 3, at 12½ h. equal to, if not a little less than, 7 Lacertæ; but the weather was not very favourable: it seemed to have increased since my first observation, which was at 5½ h.

Nov. 5, at 13 h. brighter than 7 Lacertæ, and less than ζ Cephei; flying clouds, but air pretty clear.

Nov. 6, at 9 h. and 12½ h. rather less than 7 Lacertæ.

Nov. 7, at 7½ h. I thought it still rather less than 7 Lacertæ, but at 10½ h. and 11 h. it was evidently less than it; air clear.

Nov. 10, at 11 h. and 12½ h. something less than ζ Cephei, and brighter than 7 Lacertæ; clear sky.

Nov. 11, at 7 h. to 12 h. a little brighter than 7 Lacertæ.

Nov. 12, at 7 h. and 8½ h. about equal to 7 Lacertæ. From 9½ h. to 12½ h. it was something less than 7 Lacertæ, and brighter than ε Cephei.

Nov. 13, at 6½ h. to 11 h. only a little brighter than ε Cephei, though sometimes it appeared equal to it.

Nov. 14, at 7½ h. brighter than ε Cephei, and, I believe, equal to 7 Lacertæ. There was a haziness about 7 Lacertæ.

Nov. 15, at 12 h. less than ζ Cephei, and brighter than 7 Lacertæ; fine *aurora borealis*, but the air was very clear.

At 18½ h. ditto; but the air was not very clear.

Nov. 16, at 6½ h. and 10 h. just the same, if not decreased at 10 h.

Nov. 17, at 6½ h. to 10½ h. a little less than 7 Lacertæ, and brighter than ε Cephei; air clear.

Nov. 18, at 9 h. to 12 h. and 19 h. little brighter than ε and ζ Cephei.

Nov. 19, at 6 h. to 10 h. just the same, being but a very little brighter than ϵ and ζ Cephei; air clear.

At 18 $\frac{1}{2}$ h. it was increased, being now brighter than ϵ and ζ Cephei.

Nov. 20, 7 h. to 11 h. considerably brighter than γ Lacertæ, something less than ζ Cephei; air extremely clear at 11 h.

Nov. 21, at 6 h. exactly the same.

Nov. 22, at 9 $\frac{1}{2}$ h. about equal to γ Lacertæ; moon-light.

Nov. 25, at 7 h. and 8 h. less than γ Lacertæ, and brighter than ϵ and ζ Cephei; air clear.

At 9 $\frac{1}{2}$ h. and 9 $\frac{3}{4}$ h. a little brighter than γ Lacertæ.

At 10 $\frac{1}{2}$ h. and 12 h. brighter than γ Lacertæ, and about between ζ Cephei and γ Lacertæ, but rather nearer γ Lacertæ; air clear and moon-light.

Nov. 26, at 9 h. exactly as last night.

Nov. 29, at 7 $\frac{1}{2}$ and 8 h. less than γ Lacertæ, and something brighter than ϵ and ζ Cephei.

Nov. 30, at 8 $\frac{1}{2}$ h. as last night; air clear.

At 10 $\frac{1}{2}$ h. between γ Lacertæ and ϵ Cephei, but nearer ϵ .

At 10 $\frac{1}{2}$ h. 11 h. and 12 h. ditto, but nearer γ Lacertæ; air clear. I have no doubt of its increase since 8 $\frac{1}{2}$ h. Mr. E. PIGOTT found it rather less than ζ Cephei at 18 $\frac{1}{2}$ h. *See his Observations.*

Dec. 1, 11 h. something less than ζ Cephei, and brighter than γ Lacertæ,

Dec. 3, 12 $\frac{1}{2}$ h. less than γ Lacertæ, and brighter than ϵ Cephei.

Dec. 4, 5 $\frac{1}{2}$ h. to 12 h. little brighter than ϵ and ζ Cephei.

Dec. 7, 10 h. and 11 h. between ζ Cephei and γ Lacertæ.

Dec. 8, at 10 $\frac{1}{2}$ h. between γ Lacertæ and ϵ Cephei.

Dec.

Dec. 9, 11½ h. ditto, but nearer ϵ Cephei; about equal to ξ Cephei.

Dec. 11, 6 h. something less than γ Lacertæ; brighter than ϵ Cephei.

At 7½ h. something brighter than γ Lacertæ.

At 8½ h. brighter than γ Lacertæ.

At 9 h. and 11 h. between ζ Cephei and γ Lacertæ, but nearer γ Lacertæ.

Dec. 12, at 6 h. something less than ζ Cephei.

Dec. 13, at 9½ h. brighter than γ Lacertæ; considerably less than ζ Cephei.

Dec. 14, at 8½ h. nearly equal to, if not less than, γ Lacertæ.

Dec. 17, at 5½ h. and 7½ h. equal to, if not less than, ϵ Cephei, and between ζ Cephei and γ Lacertæ, but nearer ζ .

Dec. 18, at 9 h. less than ϵ Cephei, and between ζ Cephei and γ Lacertæ, but nearer γ Lacertæ.

Dec. 19, at 19 h. less than γ Lacertæ; considerably brighter than ϵ and ξ Cephei.

Dec. 20, at 6 h. and 7 h. about equal to ξ Cephei, and a little brighter than ϵ Cephei.

Dec. 21, at 8 h. and 18 h. nearly equal to ϵ Cephei.

Dec. 22, at 8½ h. considerably brighter than γ Lacertæ, less than ζ , and a little less than ϵ Cephei; strong moon-light.

Dec. 25, at 5½ h. between γ Lacertæ and ϵ Cephei.

Dec. 28, at 8 h. &c. between ζ Cephei and γ Lacertæ, and equal to, if not less than, ϵ Cephei.

Having, in the beginning of this paper, mentioned my intention of omitting several observations, in order to be as short as possible, I have thought it best, with the exception of one only, to leave out all that were made in January, February, and

and March, because they were much interrupted by the then unfavourable state of the weather.

1785, Feb. 8, at 9 h. equal to 7 *Lacertæ*; considerably less than *Cephei*.

At 10 h. rather brighter than 7 *Lacertæ*.

At 11 h. brighter than 7 *Lacertæ*; a little less than *Cephei*.

April 1, at 11 h. about equal to α and ξ *Cephei*; weather not favourable.

April 2, at 12½ h. ditto.

April 3, at 8 h. a little less than *Cephei*, less than ζ *Cephei*, and brighter than 7 *Lacertæ*.

April 4, at 12 h. ditto; if any thing, it is less than it was last night.

April 7, at 10 h. about equal to α and ξ *Cephei*; but the weather was not very favourable.

April 8, at 7½ h. considerably less than *Cephei*, brighter than α and ξ *Cephei*; but the air was not very clear.

At 10 h. it was increased.

At 11 h. only a little less than *Cephei*.

At 12 h. equal to, if not a little brighter than α , and less than ζ *Cephei*; considerably brighter than 7 *Lacertæ*.

April 12, at 12 h. a little less than α , and nearly equal to ξ *Cephei*.

April 13, at 9 h. just the same.

At 11 h. seemed rather increased, being equal to α , and a little brighter than ξ *Cephei*.

April 16, at 11½ h. nearly equal to α and ξ *Cephei*.

April 17, at 9 h. and 11 h. rather a little less than α , and a little brighter than ξ *Cephei*.

April

April 19, at 11½ h. about equal to γ Cephei, if not a little brighter than it; less than ζ Cephei, and considerably brighter than γ Lacertæ.

April 24, at 10 h. a little brighter than γ Lacertæ; considerably less than γ Cephei.

At 12 h. scarce at all altered, but if any thing it is a little increased; air very clear, and observation good.

April 25, at 10 h. and 11½ h. little less than γ Cephei, and considerably brighter than γ Lacertæ.

April 26, at 10 h. and 11 h. less than γ Lacertæ, something brighter than ϵ , and brighter than ξ Cephei.

May 4, at 9½ h. and 12 h. a little less than ϵ and ξ Cephei.

May 7, at 12 h. less than γ Cephei, and a little brighter than γ Lacertæ.

May 9, at 11 h. a little less than ϵ Cephei.

May 10, at 12 h. between γ Lacertæ and ϵ Cephei, but something nearer ϵ .

May 11, at 10 h. and 12 h. brighter than γ Cephei, less than ζ Cephei, and much brighter than γ Lacertæ.

May 14, at 11½ h. much less than γ Lacertæ, equal to, if not a little brighter than, ϵ Cephei, and brighter than ξ Cephei.

May 15, at 9½ h. less than ϵ , and about equal to ξ Cephei.

May 19, at 9½ h. and 11 h. equal to, if not a little brighter than, ϵ Cephei, and brighter than ξ Cephei.

May 20, at 9½ h. 11 h. and 12 h. a little less than ϵ , and nearly equal to ξ Cephei.

May 21, at 12 h. equal to, if not a little less than, γ Cephei; less than ζ Cephei, and considerably brighter than γ Lacertæ.

May 22, at 10 h. and 11½ h. a little brighter than γ Cephei, the rest as last night.

May 23, at 11 h. and 11½ h. nearly equal to γ Lacertæ, and less than γ Cephei.

May 25, at 10 h. and 12 h. a little less than ϵ , and about equal to ξ Cephei.

May 27, at 10 h. between ζ and ι Cephei, and considerably brighter than 7 Lacertæ.

May 28, at 12 h. between ι Cephei and 7 Lacertæ.

June 1, at 9½ h. I thought it less than ι Cephei; air not clear, and twilight pretty strong.

At 10½ h. and 12 h. between ζ and ι Cephei, but rather nearer ι .

June 2, at 12 h. exactly the same.

June 6, at 12 h. ditto; the weather was not very favourable, but the observation seemed good.

June 10, at 11½ h. a little less than ι Cephei.

June 12, at 11 h. between ζ and ι Cephei.

June 21, at 10 h. nearly equal to, if not a very little brighter than, ι Cephei; twilight.

At 11½ h. a little less than ϵ , and about equal to ξ Cephei.

June 23, at 11½ h. between ζ and ι Cephei, and brighter than 7 Lacertæ.

June 24, at 11½ h. ditto; only a short view.

June 25, at 11½ h. a little, but certainly, brighter than ϵ Cephei, brighter than ξ Cephei, and considerably less than 7 Lacertæ.

June 26, at 11½ h. a little less than ι Cephei, and equal to, if not a little brighter than, ξ Cephei.

In the above collection I find only two or three mistakes of any consequence, viz. the dates of the observations of April 7, and 8, are marked in my journal for April 8, and 9; but I have corrected them, being convinced they are erroneous: and the observation of May 10, I think, disagrees rather too much from what it ought to be by computation.

The

The following observations were made by my friend Mr. E. PIGOTT; who, at my request, was so kind as to observe the star as often as possible, though then in an ill state of health. They are, I presume, sufficient to corroborate the variation of the star as above stated, although in one or two places there may be found some little differences between our observations.

MR. PIGOTT'S OBSERVATIONS.

1784, Oct. 25, at 12 h. rather brighter than γ Lacertæ; much brighter than ϵ Cephei, and much less than ζ Cephei; nearly between ζ Cephei and γ Lacertæ.

Oct. 26, at 12 h. seemed the same as yesterday.

Nov. 1, at 12 h. brighter than ϵ Cephei; seemed rather less than γ Lacertæ.

Nov. 13, at 8½ h. rather, but very little, brighter than ϵ Cephei; less than γ Lacertæ.

Nov. 15, at 12 h. seemed rather brighter than γ Lacertæ, and less than ζ Cephei.

Nov. 17, at 8 h. less than γ Lacertæ; rather brighter than ϵ Cephei.

Nov. 18, at 12 h. equal to ϵ Cephei, though sometimes it seemed less; less than γ Lacertæ.

Nov. 19, at 12 h. seemed equal to ϵ Cephei.

Nov. 20, at 11 h. rather less than ζ Cephei; brighter than γ Lacertæ.

Nov. 25, at 11½ h. if not equal rather brighter than γ Lacertæ; much brighter than ϵ Cephei.

Nov. 29, at 8 h. equal to ϵ Cephei.

Nov. 30, at 11½ h. brighter than ϵ Cephei; less than γ Lacertæ.

At 18½ h. much increased; rather less than ζ Cephei.

Dec. 4, at 6½ h. sometimes thought it less, and at other times brighter, than ε Cephei.

Dec. 11, at 5½ h. less than γ Lacertæ; rather brighter than ε Cephei.

At 11½ h. rather brighter than γ Lacertæ; not at its full brightness.

Dec. 21, at 7 h. if any difference less than ε Cephei.

At 18½ h. a little brighter than ε Cephei.

Dec. 22, at 8 h. less than ζ Cephei; a little brighter than γ Lacertæ.

Dec. 28, at 5½ h. nearly equal to ζ Cephei; had only a short view of them.

1785, April 26, at 11½ h. less than ζ, rather less than ε Cephei, brighter than ε Cephei, and if any difference rather brighter than γ Lacertæ.

May 4, at 9½ h. much less than ζ Cephei, less than ε Cephei, and than γ Lacertæ, and rather brighter than ε Cephei.

May 7, at 11 h. rather less than ε Cephei, and brighter than ε Cephei.

May 9, at 11½ h. rather brighter than ε Cephei, and much less than ε Cephei, and γ Lacertæ.

May 11, at 10½ h. rather less than ζ, and rather brighter than ε Cephei; much brighter than γ Lacertæ.

May 19, at 10 h. equal to ε Cephei, but if any difference rather brighter; little hazy and moon-light. The same at 12 h. but the weather was not hazy then.

May 20, at 11½ h. and 12½ h. rather brighter than ε Cephei, and much less than γ Lacertæ; moon-light strong at 11½ h.

May 21, at 12½ h. equal to γ Lacertæ; less than ε Cephei.

May

May 22, at 12½ h. equal to, if not brighter than, 7 Lacertæ; think it brighter than, Cephei.

May 23, at 11½ h. seemed sometimes equal to, though generally less than, Cephei and 7 Lacertæ.

Having now delivered the observations, from whence I have deduced the preceding conclusions, nothing more relative to this subject remains to be mentioned, except the determination of the period; in the doing of which I must follow nearly the same methods as have been used in some preceding papers. It is very evident, from a rough calculation, where only single periods or very short intervals are used, that it is about five days and eight hours. In order to determine this period with greater exactness, I have, in the following table, collated some of the most precise phases. The first five are times when δ Cephei was observed to be equal to 7 Lacertæ during the course of its increase of brightness, which proceeds rapidly. The five next are similar times, with this only difference, that as it was not then actually observed to be equal to 7 Lacertæ, a proper allowance from the nearest observations was made on supposition that the changes are similar in every period. The ten last are assumed times between its least and greatest brightness, which determinations can hardly err more than a few hours, as the whole increase is completed in thirteen hours; but even were it so, the periods deduced from them would still be exact, because the intervals are very long.

1784 and 1785.

D.	H.				D. H. M.
Nov. 25,	8 $\frac{1}{2}$	} an interval of 14 periods each of			5 8 34 $\frac{1}{2}$
Feb. 8,	9				
Dec. 11,	6 $\frac{1}{2}$	} ditto	11	ditto	5 8 54 $\frac{1}{2}$
Feb. 8,	9				
Nov. 25,	8 $\frac{1}{2}$	} ditto	25	ditto	5 8 36
April 8,	8				
Nov. 30,	16	} ditto	24	ditto	5 8 40
April 8,	8				
Nov. 30,	16	} ditto	27	ditto	5 8 35 $\frac{1}{2}$ -
April 24,	8				
Oct. 23,	21	} ditto	39	ditto	5 8 41 $\frac{1}{2}$ +
May 21,	0				
Nov. 19,	22	} ditto	34	ditto	5 8 31 $\frac{1}{2}$
May 21,	0				
Dec. 22,	1	} ditto	28	ditto	5 8 32 +
May 21,	0				
Nov. 19,	22	} ditto	25	ditto	5 8 40 $\frac{1}{2}$
April 2,	23				
Nov. 19,	22	} ditto	6	ditto	5 8 50
Dec. 22,	1				

Hence the period is, on a mean, - 5 8 37 $\frac{1}{2}$ +.

A few cursory remarks shall conclude this Paper. What I have before mentioned, that the greatest brightness of δ Cephei does not seem to be always quite the same, is not peculiar to this star, but is also to be observed in the other variable ones. I have remarked in a late Paper, that the greatest brightness of β Lyræ is subject to considerable alterations, and thought then that it might be owing to some fallacy of observation; but now

I have reason to alter, in some measure, my opinion on this head. Even Algol does not seem to be always obscured in the same degree, being perceived to be sometimes a little brighter than ϵ Persæi, and sometimes less than it *. These seeming irregularities, however, do not appear to affect the period; for if we compare the same precise phases together, it will be found still regular. This may, I suppose, be accounted for, by a rotation of the star on its axis, having fixed spots that vary only in their size.

I need not say, that the situation of δ Cephei, on account of its great northern declination is such, that its changes may be observed with great advantage in these latitudes, it being always sufficiently elevated above the horizon. To this circumstance are also owing its various changes of position, which, I find, affect the comparative brightness of the stars a little; but, as these differences are very trifling, I shall take no further notice of them.

If you think this account worthy of notice, I beg you will be so kind as to communicate it to the Royal Society.

I remain, with great regard, &c.

JOHN GOODRICKE.

* This will appear from an attentive examination of the observations of that star's diminution in my two late Papers, which were printed in the LXXIII^d and LXXIVth volumes of the Philosophical Transactions. I did not take much notice of it then, because I thought the difference was too small to be relied on; but the observations I have made since seem to confirm that it does *really* diminish a little unequally. M. MICHAIN, in a letter to Mr. E. PIGOTT, mentions the same fact.



III. *Magnetical Experiments and Observations.*

By Mr. Tiberius Cavallo, F. R. S.

(The Lecture founded by the late HENRY BAKER, Esq. F.R.S.)

Read November 24, 1785.

THE object of this lecture is to shew the properties of some metallic substances with respect to magnetism; and the experiments herein related seem to ascertain some new and remarkable facts,

The magnetic properties have been generally thought to belong only to iron, or to those substances which contained that metal; comprehending under the general name of iron not only the metal commonly so called, but likewise its more perfect and more imperfect states, *viz.* steel, iron ores, amongst which is considered the magnet, and the calces of iron, excepting only those which are very much dephlogisticated, for they possess no magnetic property whatsoever. Some other metallic substances, and especially platina, brass, and nickel, on which the magnet has some action, were thought to be magnetic so far as they contained some portion of iron, the presence of which may be manifested by chemical methods in many cases, but not always; because the quantity of iron may be so excessively small in proportion to the weight of the other metal in which it is concealed, as not to be discoverable by chemical analysis, and yet it may be sufficient to affect the magnetic needle.

needle. The following experiment will shew, that an exceedingly small quantity of iron will render a body sensibly magnetic.

Having chosen a piece of Turkey-stone, which weighed about an ounce, I examined it by a very sensible magnetic needle, and found that it had not the least degree of magnetism, the needle not being moved from its usual direction by the vicinity of any part of the surface of the stone; I then weighed a piece of steel with a pair of scales that turned with the twentieth part of a grain, and afterwards drew one end of it over the surface of the stone in various directions. This done, the piece of steel was weighed again, and was found to have lost so small a part of its weight as not to be discernible by that pair of scales; yet the Turkey-stone, which had acquired only that small quantity of steel, affected the magnetic needle very sensibly. Chemistry seems not to afford any means by which so small a quantity of iron may be decisively detected in a body that weighs one ounce. Hence it follows, that though no iron is to be discovered in a body by chemical methods, yet it should not be concluded, that the said body, if it affect the magnetic needle, does not own its magnetism to some small quantity of iron concealed in its substance.

Nickel is a metallic substance which has been suspected to be capable of acquiring some degree of magnetism independent of iron; and this suspicion has been founded upon observing, that nickel retained its magnetism after having been repeatedly purified*. There are, however, persons who have denied the magnetism of purified nickel; and I have seen some pieces of it which did not in the least affect the magnetic needle. It is probable, that those pieces were not pure nickel, and perhaps

* See KIRWAN's Mineralogy, p. 342. and 367.

some cobalt was contained in them; but I see no reason why the nickel, when alloyed with a little cobalt, should shew no magnetism, if that property does really belong essentially to it.

The greatest number of my experiments are relative to the properties of brass; and they seem to prove, that this compound metal, which is often magnetic, does not owe its magnetism to iron, but to some particular configuration of its component particles, occasioned by the usual method of hardening it, which is by hammering.

In some specimens of brass, and especially in that which has often passed from the work-shop to the furnace, and from the latter to the former, there are sometimes pieces of iron sensible not only to the magnet, or the chemical analysis, but even to the sight, which render the brass strongly magnetic. But the brass generally used in my experiments was such as, when quite soft, it had no sensible degree of magnetism.

Before we begin with the narration of those experiments, it will be proper to describe the magnetic needle I generally used, which is suspended in a particular manner; and which may be useful to persons who are fond of making magnetic experiments, not only for its sensibility, but likewise for the simplicity of its construction.

Experience having shewn, that large magnetic needles are not proper for experiments wherein a very small degree of magnetism must be ascertained, and the free motion of the usual small needles being proportionally more obstructed by the nature of their suspension, even when furnished with agate caps, I endeavoured to contrive a sort of suspension which might answer the purpose better than the needles suspended in the usual manner; and, after several attempts, at last I constructed a chain

of

of horse-hair, consisting of five or six links, to which the needle was suspended. Each link is about three-quarters of an inch in diameter; and the extremities of each piece of hair, which is formed in a ring, are joined by a knot, and secured by a little sealing-wax. The link on one end of this chain is suspended to a pin in a proper frame, or any support that may be at hand; and to the link of the other extremity which lies lowermost, a piece of fine silver wire is hooked. This wire is about an inch and a half long, and its lower extremity is fastened round a small and cylindrical piece of cork, through which a common sewing needle, made magnetic, is thrust horizontally. Thus the magnetic needle is suspended by a hair-chain, the links of which, on account of the smoothness and lightness of the hair, move very freely in each other, and allow the needle more than a whole revolution round its centre, with so small a degree of friction as may be considered next to nothing. By comparing this needle with others of the best sort in use, I find the former to be much more sensible; for when bodies which have an exceedingly small magnetic power are tried, this needle will be frequently attracted by them when the others are not sensibly affected.

In order to try farther the delicacy of such suspension, I placed a piece of looking-glass under the needle, and nearly horizontal, so that the image of the needle was seen in it. Now, as a fine line had been previously marked on the glass, things were so disposed as that the image of the needle might coincide with the line marked on the glass, the eye being placed in a proper point of view; afterwards, by shaking the needle either very gently or very quickly, I repeatedly endeavoured to place it out of the magnetic meridian; but every

Vol. LXXVI. K endeavour

endeavour proved ineffectual, for the needle constantly settled in the same direction, without any sensible variation.

With a needle thus suspended a variation compass might be very easily constructed, and it would perhaps be more accurate than those commonly in use. For this purpose the needle ought to be about three inches long, and the piece of looking-glass ought to be fixed upon the index of an HADLEY's sextant, which must be placed horizontally under the needle, with its edge or fiducial line in the meridian of the place, in order to observe the daily variation of the needle. I have made only a rough model of such a variation compass, and it seemed to answer very well. This construction appears to have the following advantages over the common sort: 1st, the needle being cylindrical, and without a hole in the middle, would be less subject to have more than two poles. 2dly, The needle being slender, its poles would stand more exactly in its axis, which with the common flat needles is seldom the case. 3dly, It will appear, by a little consideration, that in this construction there is no need of the needle's center of motion keeping always in the same invariable point, which renders the construction both very easy and very accurate: and, lastly, as the sextant may be placed at a considerable distance below the needle, and the rest of the frame may be made of any size, there would be no necessity of placing any brass or other metal so near the needle as might affect it in case this metal had any magnetism, which generally happens with brass.

In order to examine the magnetism of divers substances, besides the above described needle, I used to put a small magnetic needle upon water, and then bring the substance to be examined near it, or place the substance itself upon water, sometimes

sometimes resting it upon pieces of cork, and then bring a powerful magnet near it.

Examination of the Magnetical Properties of Brass.

A few years ago, being intent on making some magnetic experiments, in which brass was concerned, I used to examine first whether the pieces of brass had any magnetism or not, and rejected those pieces which had an evident degree of that power. In the course of those experiments I remember to have observed, that those pieces of brass which had been hammered were generally magnetic, and much more so than others; in consequence of which I made no use of hammered brass in those experiments. But lately, having ordered a theodolite at a philosophical instrument shop, I particularly enjoined the workmen to try the brass, both soft and hammered, before they worked it, and to make no use of that which had any magnetism. They found, that hammered brass, even such as before the hammering had no magnetism, could afterwards disturb the magnetic needle very sensibly. These observations induced me to make the following experiments.

E X P E R I M E N T I.

An oblong piece of brass, weighing somewhat less than half an ounce, being examined by presenting every part of its surface to the suspended needle, shewed no sign of magnetism whatever. It was then hammered for about two minutes; the consequence of which was, that it became magnetic so far as to attract either end of the needle from about a quarter of an inch. This same piece of brass being now put into the fire so as

to become red-hot, by which means it was softened, and when cold being presented to the suspended needle, its magnetism was found to be entirely gone. Hammering made it again magnetic. Softening by fire took the magnetism away a second time; and thus the magnetism was repeatedly given it by hammering, and was destroyed by softening; sometimes shewing to have acquired a sensible degree of that power, even after two or three strokes of the hammer.

EXPERIMENT II.

The result of the first experiment would naturally induce one to suspect, that the hammer and anvil might have imparted some small quantity of steel to the brass, which rendered it magnetic; and that this magnetism was destroyed in softening the brass, insomuch as the fire calcined the small quantity of steel that had adhered to it. In consequence of which consideration, I took other pieces of brass besides that used before, and hammered them between card-paper, changing the pieces of paper as often as was necessary, since they were easily broken by the hammer; but the pieces of brass became constantly magnetic by the hammering, and their magnetism was destroyed by fire.

In this experiment I generally gave to the brass not above thirty strokes with the hammer.

EXPERIMENT III.

Still suspecting that the hammer and the anvil might have imparted some small quantity of iron to the brass, because the pieces of card-paper sometimes were broken by the first or
second

second stroke, in which case either the hammer or the anvil touched the brass; I hardened a piece of brass by beating it between two large flints, *viz.* using one for the hammer, and the other for the anvil. The piece of brass became magnetic, though in this case it seemed to have acquired not so much power as when it had been hardened with the hammer; but it must be observed, that the flints being rough and irregular, the piece of brass could not be hardened by them so easily, or so equally, as by the other method.

The flints, being examined both before and after the experiment, were found to have not the least degree of magnetism.

EXPERIMENT IV.

A piece of brass, which by hammering had been rendered so strongly magnetic as to attract either pole of the needle from about a quarter of an inch, was put into a crucible, together with a considerable quantity of charcoal dust, which surrounded it every where. The crucible was covered with clay, and being put into the fire, was kept red-hot for about ten minutes. After cooling, the piece of brass was taken out of the crucible, and being examined, was found to have entirely lost its magnetism. The object of this experiment was to ascertain whether the loss of magnetism, in a piece of brass that was softened, was owing to the calcination of the ferrugineous particles, which, notwithstanding the preceding experiments, might still be suspected to be imparted to it; because in this way of softening the brass, the ferrugineous particles being surrounded with charcoal dust, could not have been calcined; hence the brass ought not to have lost its magnetism, which was not the result of the experiment.

E X P E R I M E N T V.

One of those pieces of brass which had been used for the foregoing experiments, and which had been deprived of magnetism by fire, was hammered between two large and pretty thick pieces of copper, which were not in the least magnetic; and, after a few strokes of the hammer, it became sensibly magnetic.

E X P E R I M E N T VI.

In order to examine the difference of this property in brass of various kinds, I have tried a great many pieces of English as well as foreign brass; some of which was very old, and so fine and uniform, that an eminent watch-maker of my acquaintance used it for the very best sort of watch work. But I find, that they mostly have the property of becoming magnetic by hammering, and of losing that power when softened. There are, however, some pieces which acquire no magnetism by the hammering, though they are rendered equally hard by it as those which acquire the magnetism. By attentively examining them, I have not yet been able to distinguish, without a trial, which pieces are capable of acquiring magnetism, and which not; the colour, apparent texture, and degree of ductility seeming to afford no sure indication. In short, what I have observed relating to the magnetic properties of brass is:

1st, That most brass becomes magnetic by hammering, and loses the magnetism by annealing or softening in the fire.

2dly, That the acquired magnetism is not owing to particles of iron or steel imparted to the brass by the tools employed.

3dly,

3dly, Those pieces of brass which have that property, retain it without any diminution after a great number of repeated trials, viz. after having been repeatedly hardened and softened. But I have not found any means to give that property to such brass as had it not naturally.

4thly, A large piece of brass has generally a magnetic power somewhat stronger than a smaller piece; and the flat surface of the piece draws the needle more forcibly than the edge or corner of it.

5thly, If only one end of a large piece of brass be hammered, then that end alone will disturb the magnetic needle, and not the rest.

6thly, The magnetic power which brass acquires by hammering has a certain limit, beyond which it cannot be increased by farther hammering. This limit is various in pieces of brass of different thickness, and likewise of different quality.

7thly, Though there are some pieces of brass which have not the property of being rendered magnetic by hammering; yet all the pieces of magnetic brass, that I have tried, lose their magnetism by being made red-hot, excepting indeed when some piece of iron is concealed in them, which sometimes occurs; but in this case, the piece of brass, after having been made red-hot and cooled, will attract the needle more forcibly with one part of its surface than with the rest of it; and hence, by turning the piece of brass about; and presenting every part of it successively to the suspended magnetic needle, one may easily discover in what part of it the iron is lodged.

From those observations it follows, that when brass is to be used for the construction of instruments wherein a magnetic needle is concerned, as dipping needles, variation compasses, &c. the brass should be either left quite soft, or it should be

chosen.

chosen of such a sort as will not be made magnetic by hammering, which sort however does not occur very easily.

Examination of the Magnetic Properties of some other Metallic Substances.

The result of the experiments on brass induced me to examine other metallic substances, and especially its components, viz. copper and zinc: though the result of the experiments has not been very remarkable, excepting with platina, which metal has properties in great measure analogous to those of brass.

Having examined various pieces of copper, by means of the suspended magnetic needle, and having never found them magnetical, except only sometimes in such places which had been filed, and where some particles of steel might have been left by the file, I next proceeded to hammer some pieces of it, not only in the usual way, but likewise between flints: the result, however, was very dubious; for though, in general, they had no effect whatever on the needle, yet sometimes I thought the needle was really attracted by some pieces of hammered copper; but then this attractive power was so exceedingly small as not to be depended upon.

Zinc, either not hammered, or hammered as far as could be done without breaking it, shewed no signs of magnetism whatever, when presented to the magnetic needle. A mixture of zinc and tin neither had any action upon the needle.

A piece of a broken reflector of a telescope, which consisted of tin and copper; a mixture of tin, zinc, and a little copper; a piece of silver, both soft and hammered; a piece of pure gold, both soft and hammered; a mixture of gold and silver, both hard and soft; and another mixture of a great deal of silver,

silver, a little copper, and a less quantity of gold, either before or after hammering, had not the least action on the magnetic needle.

Platina was the metal I last examined, and the experiments made with it seem to deserve particular attention.

EXPERIMENT I.

A large piece of platina, which, after being precipitated from its solution in *aqua regia*, had been fused, or rather concreted together, being presented to the suspended magnetic needle, shewed not the least sign of magnetism. It was then hammered; but after the third or fourth stroke of the hammer it broke into many pieces, several of which being tried, shewed no magnetism, nor could any of the finest particles be attracted by the magnet presented very nearly over them. The broken surface of this piece of platina was full of cavities, some of which were large, and others just discernible; and altogether the metal seemed to have undergone an imperfect fusion.

EXPERIMENT II.

The grains of native platina were examined next, by putting a magnet just over them; but the magnet attracted not above ten or twenty particles out of about half an ounce of platina: and those which were attracted had either little or no shining metallic appearance like the rest, and were exceedingly small.

EXPERIMENT III.

Having picked out several of the largest grains of platina, I presented the magnet to them; but they were not

in the least attracted by it. One of those grains was then hammered; by which means, after about eight or ten strokes, it was spread into a plate, about a tenth of an inch in diameter, and nearly circular; afterwards the magnet being presented to it, the former attracted it from the distance of about one-twentieth of an inch. The other grains being all hammered one after the other, were rendered by it so far magnetic as to be attracted by the magnet, and to disturb the suspended needle when they were presented to it. But there were some amongst them which acquired no magnetism at all, though they had been purposely hammered much longer than the others.

As far as I could observe, those pieces which would not acquire any magnetism by hammering, had not a very shining appearance before the hammering, though afterwards they could not be distinguished from the others by their appearance; and they seemed not to spread under the hammer so easily as the others.

In general three or four strokes are sufficient to render a grain of platina evidently magnetic, but about ten strokes give it the full power it is susceptible of.

EXPERIMENT IV.

Those grains of platina, which in the preceding experiments had been rendered magnetic by hammering, being put upon a charcoal, were made red-hot by means of a blow-pipe; and afterwards being presented to the magnet, and likewise to the suspended needle, they shewed not the least sign of magnetism. Heat, therefore, deprives them as well as brass of the magnetism acquired by hammering. A second hammering rendered them magnetic, though not so quickly, nor to so great a degree,

degree, as it had done the first time. However, it must be observed, that the pieces of platina having been rendered flat and thin by the first hammering, could not be so easily struck, nor spread much more, by the second.

If it is true, as those experiments seem to prove beyond a doubt, that magnetism may exist, or may belong to other substances, independent of iron, it must follow, that the attraction of a few particles of an unknown substance by the magnet is not a sure sign of the presence of iron. Hence those substances, which hitherto have been considered as containing ferruginous particles, for no other reason but because the magnet attracted a small quantity of them, must be considered as dubious; and the conclusion of the existence of iron should not be admitted, except when those particles, which have been separated by the magnet, appear to be iron by some other trial; for though it is true, that iron is always attracted by the magnet, yet it does not hence follow, that whatever is attracted by the magnet must be iron.

P O S T S C R I P T.

THE existence of magnetism, or of the power of attracting and being attracted by the magnetic needle, in bodies, without the interference of iron or any ferruginous matter, being a proposition not only new and singular, but seemingly of importance in philosophy, the experiments which tend to confirm it should be never deemed superfluous, nor any possible objection be left unanswered: hence, since the writing of the foregoing paper, I have endeavoured to raise objections, and to con-

trive means of explaining them; but every consideration seemed to confirm the proposition advanced. The principal of those objections was, that the brass which becomes magnetic by hammering and loses that power by softening, might contain a small quantity of iron, to which that magnetic power was owing; and that this iron or martial earth, dispersed through the substance of the brass, might become phlogisticated by the action of hammering; inasmuch as the brass being forced into a smaller space might perhaps give some of its phlogiston to the martial earth, and thus render it magnetic; and, on the contrary, the action of the fire in softening might remove that phlogiston from the martial earth, and give it to the brass; hence the former, remaining quite dephlogisticated, would no longer shew any signs of magnetism. The consideration that iron may be dephlogisticated or calcined more easily than brass gave an apparent weight to the supposition; but the following experiments seem to expel every doubt.

EXPERIMENT I.

Having chosen a piece of brass which would acquire no magnetism by hammering, I placed it upon the anvil, together with a considerable quantity of *crocus martis*, which crocus had no action on the magnetic needle; then began hammering the brass, and turning it frequently, in order to let part of the crocus adhere to it; and, in fact, the crocus had in several places been fastened so well into the brass, that hard wiping with a woollen cloth would not rub it off. The brass appeared red in those places; but, after having been hammered for a long time, it acquired no magnetism whatever. The hardening, there-
fore,

fore, could not render the iron calx so far phlogisticated as to affect the magnetic needle.

EXPERIMENT II.

In order to diversify the preceding experiment, I drilled a hole, about one-eighth of an inch long, and little more than one-fiftieth of an inch in diameter, into a piece of brass that was not rendered magnetic by hammering, and filled it with *crocus martis*; then I hammered the piece of brass, thus inclosing the calx of iron, and afterwards presented it to the needle; but there was not the least sign of attraction: the martial earth, therefore, had not acquired any phlogiston from the brass by the action of hammering.

EXPERIMENT III.

The piece of brass mentioned in the preceding experiment, viz. with a little calx of iron in it, was put into the fire, and was made quite red-hot, in which state it remained for about three minutes. Then, after cooling, it was presented to the magnetic needle, and this was attracted by the brass only in that place wherein the calx of iron was contained. The action, therefore, of the fire had rendered the martial earth so far phlogisticated as to attract the magnetic needle; hence, if the magnetism of brass was owing to any ferrugineous matter contained in it, a piece of brass ought to become magnetic when softened, which is contrary to the experiments mentioned in this paper.

EXPERIMENT IV.

A hole, similar to that mentioned in the second experiment, was drilled into a piece of brass that would not become magnetic by hammering, and into it was put some black calx of iron, which was so far phlogisticated as to be attractable by the magnet, and the hole was closed by a few strokes of the hammer. In consequence of which the piece of brass, when presented to the suspended magnetic needle, would attract it only about that place where the magnetic calx was contained. This attraction was very weak. Then the piece of brass, thus prepared, was put into the fire, and was kept for about six minutes, in a heat very little short of that necessary to melt brass, and after cooling I presented it to the needle, expecting that the fire might have dephlogisticated that calx of iron so far as not to let it act any longer upon the needle; but the attraction appeared to be of the same degree it was before the heating.

It seems, therefore, to be demonstrated, as far as the subject will admit of demonstration, that the magnetism acquired by brass, when hammered, is not owing to iron contained in it; and, consequently, that *magnetism, or the power of being attracted by, and attracting, the magnet, may exist independent of iron.*

TO DR. BLAGDEN, SEC. R. S.

S I R,

Windsor, January 9, 1786.

I HAVE made the experiment which you recommended me to try, relating to the magnetism of brass; *viz.* I mixed, by means of the blow-pipe, a small quantity of iron, with about four times its weight of such brass as would not become magnetic by hammering. The whole globule weighed about two grains, and it attracted the magnetic needle very powerfully. I then melted this globule of brass and iron with about fifty grains of the same sort of brass as had been used before. After cooling, the whole lump of brass appeared to have very little power upon the magnetic needle, every part of its surface attracting one end of the suspended needle, so as to let it just adhere to it when the air was not at all disturbed. But this weak and hardly perceivable degree of magnetism was not increased by hammering, nor annihilated by softening.

In the course of my experiments on the magnetism of brass, I have twice observed the following remarkable circumstance. A piece of brass, which had the property of becoming magnetic by hammering, and of losing the magnetism by softening, having been left in the fire till it was partially melted, I found, upon trial, that it had lost the property of becoming magnetic by hammering; but having been afterwards fairly melted in a crucible, it thereby acquired the property it had originally, *viz.* that of becoming magnetic by hammering.

I have

I have likewise often observed, that a long continuance in a fire so strong as to be little short of melting hot, generally diminishes, and sometimes quite destroys, the property of becoming magnetic in brass. At the same time, the texture of the metal is considerably altered, becoming what some workmen call *rotten*. From this it appears, that the property of becoming magnetic in brass by hammering, is rather owing to some particular configuration of its parts, than to the admixture of any iron; which is confirmed still farther by observing, that Dutch plate-brass (which is made not by melting the copper, but by keeping it in a strong degree of heat whilst surrounded by *lapis calaminaris*) also possesses that property; at least all the pieces of it, which I have tried, have that property.

I am, &c.

T. CAVALLO.



IV. On Infinite Series. By Edward Waring, M. D. F. R. S.
Lucasian Professor of Mathematics at Cambridge.

Read December 15, 1785.

1. **I**N the Paper, which the Royal Society did me the honour to print, on Summation of Series, is given a method of finding the sum of a series, whose general term $\left(\frac{P}{Q}\right)$ (where

$\frac{P}{Q}$ is a fraction reduced to its lowest terms) is a determinate algebraical function of the quantity (z) the distance from the first term of the series, which always terminates when the sum of the series can be expressed in finite terms.

2. The terms of every infinite series must necessarily be given by a function of z , or by quantities which can be reduced to a function of z .

3. Let $Q = A \times A' \times A'' \times \dots \times A^{(n)} \times B \times B' \times B'' \times \dots \times B^{(m)} \times C \times C' \times C'' \times \dots \times C^{(p)} \times \&c.$ where $A', A'', A''', \dots, A^{(n)}$, are successive values of A ; that is, result from A by writing in it for z respectively $z+1, z+2, z+3, \dots, z+n$; and $B', B'', B''', \dots, B^{(m)}$, result from B , by writing in it for z respectively $z+1, z+2, z+3, \dots, z+m$; but B is not a successive value of A ; &c. Let the numerator $P = E \times E' \times E'' \times \dots \times E^{(r)} \times F \times F' \times F'' \times \dots \times F^{(s)} \times L \times E' \times E'' \times \dots \times E^{(t)} \times F' \times F'' \times \dots \times F^{(u)} \times \&c.$ denoting successive values of the quantities $E, F, \&c.$ respectively; and L , admitting of no divisor of the formula $K \times K'$, where K' is a suc-

cessive value of K : let $L = A \times B \times C \times \&c. \times E^b \times F^c \times \&c. \times p' \times q' \times r' \times \&c. - A^m \times B^n \times C^r \times \&c. \times E \times F \times \&c. \times p \times q \times r \times \&c.$ where $p', q', r', \&c.$ are irrational quantities and successive values of $p, q, r, \&c.$ The factors $A, B, C, \&c. E^b, F^c, \&c.$ being given, the factors $p', q', r', \&c.$ into which they are multiplied in the quantity L will easily be deduced by deducting the preceding irrational factors contained in $A, B, C, \&c. E^b, F^c, \&c.$ from the correspondent irrational factors contained in L ; and in the same manner, from the factors $A^m, B^n, \&c. E, F, \&c.$ can be deduced the irrational factors of the preceding $p, q, r, \&c.$

Assume for the sum of the series sought the quantity

$$\frac{E \times E' \times E'' \dots E^{b-1} \times F \times F' \dots F^{c-1} \times \&c.}{A \cdot A' \cdot A'' \dots A^{m-1} \times B \times B' \times B'' \dots B^{n-1} \times C \times C' \dots C^{r-1} \times \&c. \times p \times q \times r \times \&c. (ax^m + \beta x^{m-1} + \gamma x^{m-2} + \&c.) = V;}$$

where m is a whole number, and $\alpha, \beta, \gamma, \&c.$ are co-efficients to be investigated; write in V for x its successive value $x+1$, and let the result be W ; reduce the difference $W - V$ into a fraction in its lowest terms; and make the co-efficients of the correspondent terms of the resulting fraction $= (W - V)$ and of the given fraction equal to each other, if possible; and thence may be deduced the sum of the series required.

4. This series will terminate if the sum sought can be expressed by a finite determinate function of x ; if not, it will proceed in infinitum, and may be expressed either by a series ascending or descending according to the dimensions of x .

5. If any factor, A or B , or $C, \&c.$ have no successive one in the denominator; or if the greatest dimensions of x in the denominator be greater than its greatest dimensions in the numerator by 1, then the sum of the series is not a finite algebraical function of x .

6. If in the denominator are deficient some intermediate successive factors, multiply both the numerator and denominator by those deficient factors, and they are supplied: for example, let $A \times A''' \times A'''' \times \&c.$ be factors of the denominator, in which are deficient the factors A' , A'' , A''' , &c. multiply both numerator and denominator by the content $A' \times A'' \times A''' \times \&c.$ and they are restored.

7. If in the denominator is contained the content $A' \times A'' \times A''' \dots A'^{\lambda} = H$, in which λ is the least of the indices $l, l', l'', \&c.$: assume $A^{\lambda} \times A^{\lambda} \times A'^{\lambda} \dots A'^{\lambda} \times \alpha = H$, and in the same manner reduce the factors in α ; then assume for the denominator $A^{\lambda} \times A^{\lambda} \times A'^{\lambda} \dots A'^{\lambda-1} \times \&c.$: for example, let the contents be $A^3 \times A'^3 \times A''^3 \times A'''^3 \times \&c. = H$, then is 3 the least index, and consequently the content reduced as before taught will be $A^3 \cdot A'^3 \cdot A''^3 \cdot A'''^3 \times \alpha = A^3 \cdot A'^3 \cdot A''^3 \cdot A'''^3 \times A \cdot A' \cdot A'' \times A'^3 \cdot A''^3$; but between the factors A' and A''' is deficient the factor A'' ; and between the factors A'^3 and A'''^3 is deficient the factor A''^3 ; multiply the numerator and denominator of the given fraction by the deficient factors $A'' \times A''^3$; assume for the denominator $A^3 \cdot A'^3 \cdot A''^3 \times A \cdot A' \cdot A'' \times A'^3 \cdot A''^3 \times \&c. = A^4 \cdot A'^4 \cdot A''^4 \times \&c. \&c.$

8. If the greatest index of the content H is contained in one factor only, then the sum of the series cannot be expressed in finite terms of the quantity z .

9. The same may be applied to the contents of the several successive values of the quantities $B, C, \&c.$ in the denominator: for example, let the general term be $\frac{1}{1 \cdot 2 \cdot 3 \dots z-2 \times z}$; multiply it into $z-1$ to compleat the deficient term, and it results

$\frac{x-1}{1.2.3\dots x}$; assume, by the preceding method for the sum of the series the quantity $\frac{1}{1.2.3\dots x-1}$, of which the successive term is $\frac{1}{1.2.3\dots x}$, and their difference $\frac{1}{1.2\dots x-1} - \frac{1}{1.2\dots x} = \frac{1}{1.2\dots x-2 \times x}$ the given term.

2. Let the term be Nx^z and z less than 1, which is the term of a geometrical series; then will the sum of the infinite series be $\frac{N}{1-x} \times x^z$, beginning from the term whose distance from the first is z ; for the difference between the two successive sums $= \frac{N}{1-x} (x^z - x^{z+1}) = Nx^z$ the given term.

3. Let the general term be $\frac{(x+n+1-x^2 \times x+1)^2}{x+1 \cdot x+n+1} \times x^{z+1}$; assume for the sum of the series the subsequent quantity $(x+1 \cdot x+2 \cdot x+3 \dots x+n)^{-1} \times x^z \times (\alpha + \beta x + \gamma x^2 \dots x^{n-1})$ and by the preceding method the co-efficients α, β, γ , &c. may be found: the sum is known to be $= \frac{\alpha}{x+1} \times x^{z+1} + \frac{\alpha}{x+2} \times x^{z+1} + \frac{\alpha}{x+3} \times x^{z+1} \dots \frac{\alpha}{x+n} \times x^{z+1}$, which can easily be reduced to the preceding formula.

If the general term be $\frac{T'-T}{T \times T'}$ or $\frac{PQ'-QP'}{QQ'}$; where T and T' , P and P' , Q and Q' , are successive terms; then will the sums of the series be $\frac{1}{T}$ or $\frac{P}{Q}$ properly corrected.

10. If the function expressing the general term contain in the denominator a factor or factors, which have no successive one; reduce the factor or factors into an infinite series proceeding according to the dimensions of x , and thence, by the method before given, find the sum of the series. The same method

may be pursued, when the denominator of a fluxion, which is a function of x multiplied into $\dot{x}$ contains the simple power only of a factor or factors; reduce the factor or factors into an infinite series, proceeding according to the dimensions of x , and by the known methods find the fluent of the fluxion.

11. The fluent of the fluxion or sum of the series may be deduced also from the subsequent propositions, from which may be investigated many serieses, whose sums are known.

1. Let $p\dot{P} = Q\dot{q}$, $q\dot{Q} = R\dot{r}$, $r\dot{R} = S\dot{s}$, $s\dot{S} = T\dot{t}$, &c.; then $\int P\dot{p} = Pp - Qq + Rr - Ss + Tt - \&c.$ if only the series converges.

2. Let $pP' + p'P = Qq'$, $qQ' + q'Q = Rr'$, $rR' + r'R = Ss'$, $sS' + s'S = Tt'$, &c. where P' , p' , Q' , q' , &c. denote the increments of the quantities P , p , Q , q , &c. respectively, then will the integral of the increment $(Pp') = Pp - Qq + Rr - Ss + \&c.$ if only the series converges.

Ex. 1. $\int x^{m-n} \dot{x} = \int \frac{x^{m-n}}{x^{n-1}} = \frac{1}{x^{n-1}} \left(\frac{1}{m+1} + \frac{n+m}{r+1} A + \frac{n+r}{s+1} B + \frac{n+s}{t+1} C + \frac{n+t}{T+1} D + \&c. \right) = \frac{1}{1-nx^{n-1}}$; whence $\frac{1}{1-n} = \frac{1}{m+1} + \frac{n+m}{r+1} \times \frac{1}{m+1} \times \frac{n+r}{r+1} \times \frac{n+m}{m+1} \times \frac{1}{m+1} + \&c.$ In this example $\dot{p} = x^{m-n} \dot{x}$, $P = x^{m-n}$, $\dot{q} = x^{n-1} \dot{x}$, $Q = x^{n-1}$, $\dot{r} = x^{n-1} \dot{x}$, &c.

Ex. 2. $\int \frac{x^{m-n}}{x^{n+r}} \dot{x} = \int x^{m-n-r} \dot{x} = \frac{1}{m-n-r+1} x^{m-n-r+1} = x^{m-n-r+1} \left(\frac{1}{m+1} + \frac{1}{m+1} \times \frac{n+r}{m+2} + \frac{1}{m+1} \cdot \frac{n+r}{m+2} \cdot \frac{n+r+1}{m+3} + \&c. \right)$; whence $\frac{1}{m-n-r+1} = \frac{1}{m+1} + \frac{n+r}{m+2} A + \frac{n+r+1}{m+3} B + \&c.$ In both these examples the letters A , B , C , &c. denote the preceding terms.

Ex. 3. $\int \frac{x^{m-n}}{1+x} \dot{x} = \frac{x^{m-n}}{1+x} \left(\frac{1}{m+1} + \frac{1}{m+1} \times \frac{x}{1+x} + \frac{2}{m+1} \times \frac{x^2}{(1+x)^2} + \frac{3}{m+1} \times \frac{x^3}{(1+x)^3} + \&c. \right)$

Ex.

$$\text{Ex. 4. } \int \frac{a'x^{n+1}}{(a+bx'+cx^{2n})^{b+1}} = \frac{1}{m+1} \frac{a'x^{n+1}}{(a+bx'+cx^{2n})^{b+1}} + \frac{a'x^{n+1}}{m+1} \frac{a'x^{n+1}}{(a+bx'+cx^{2n})^{b+1}} \\ \times \left(\frac{nbbx^{n-1}}{m+n+1} + \frac{2nbccx^{2n-1}}{m+2n+1} \right) + \&c.$$

12. Let the general term of an infinite series be

$$\frac{A}{z+a \cdot z+a+1 \cdot z+a+2 \dots z+a+n \times z+\beta \cdot z+\beta+1 \cdot z+\beta+2 \dots z+\beta+}$$

$$\frac{m \times z+\gamma \cdot z+\gamma+1 \cdot z+\gamma+2 \dots z+\gamma+r \times z+\delta \cdot z+\delta+1 \cdot z+\delta+2 \dots}$$

$\frac{1}{z+\delta+s \times \&c.}$; where $\alpha-\beta$, $\alpha-\gamma$, $\beta-\gamma$, &c. are not whole

numbers; the sum of the series can always be expressed

in finite terms; if the sum of the fractions $\frac{1}{1 \cdot 2 \cdot 3 \dots n} \times$

$$\frac{1}{\beta-a \cdot \beta-a+1 \cdot \beta-a+2 \dots \beta-a+m \times \gamma-a \cdot \gamma-a+1 \cdot \gamma-a+2 \dots \gamma-a+r}$$

$$\times \delta-a \cdot \delta-a+1 \cdot \delta-a+2 \dots \delta-a+s \times \&c. - \frac{1}{1 \times 1 \cdot 2 \cdot 3 \dots n-1} \times$$

$$\frac{1}{\beta-a-1 \cdot \beta-a \cdot \beta-a+1 \cdot \beta-a+2 \dots \beta-a+m-1 \times \gamma-a-1 \cdot \gamma-a \cdot \gamma-a+1}$$

$$\dots \gamma-a+r-1 \times \delta-a-1 \cdot \delta-a \cdot \delta-a+1 \dots \delta-a+s-1 \times \&c. +$$

$$\frac{1}{1 \cdot 2 \times 1 \cdot 2 \cdot 3 \dots n-2} \times \frac{1}{\beta-a-2 \cdot \beta-a-1 \cdot \beta-a \cdot \beta-a+1 \dots \beta-a+n-2 \times \gamma-a}$$

$$-2 \cdot \gamma-a-1 \cdot \gamma-a \dots \gamma-a+n-2 \times \delta-a-2 \cdot \delta-a-1 \cdot \delta-a \dots \delta-a+n-2 \times \&c.$$

$$- \frac{1}{1 \cdot 2 \cdot 3 \times 1 \cdot 2 \dots n-3} \times \frac{1}{\beta-a-3 \cdot \beta-a-2 \cdot \beta-a+n-3 \times \gamma-a-3 \cdot \gamma-a}$$

$$-2 \dots \gamma-a+n-3 \times \delta-a-3 \cdot \delta-a-2 \cdot \delta-a+n-3 \times \&c. + \dots \frac{1}{1 \cdot 2 \cdot 3 \dots n} \times$$

$$\frac{1}{\beta-a-n \cdot \beta-a-n+1 \dots \beta-a-n+m \times \gamma-a-n \cdot \gamma-a-n+1 \dots \gamma-a-n+r \times}$$

$$\delta-a-n \cdot \delta-a-n+1 \dots \delta-a-n+s \times \&c. = 0; \frac{1}{1 \cdot 2 \cdot 3 \dots m} \times \frac{1}{a-\beta \cdot a-\beta+}$$

$$1 \cdot a-\beta+2 \dots a-\beta+m \times \gamma-\beta \cdot \gamma-\beta+1 \dots \gamma-\beta+r \times \delta-\beta \cdot \delta-\beta+1$$

$$\delta-\beta+2 \dots \delta-\beta+s \times \&c. - \frac{1}{1 \times 1 \cdot 2 \cdot 3 \dots m-1} \times \frac{1}{a-\beta-1 \cdot a-\beta \dots a-\beta+}$$

$$\begin{aligned}
 & \frac{1}{a-1 \times \gamma-\beta-1, \gamma-\beta, \gamma-\beta+r-1 \times \delta-\beta-1, \delta-\beta, \delta-\beta+s-1 \times \&c.} + \\
 & \frac{1}{1.2 \times 1.2.3 \dots m-2} \times \frac{1}{a-\beta-2, a-\beta-1, a-\beta, a-\beta+n-2 \times \gamma-\beta-2} \\
 & \frac{1}{\gamma-\beta-1, \gamma-\beta+r-2 \times \delta-\beta-2, \delta-\beta-1, \delta-\beta+s-2 \times \&c.} \\
 & \frac{1}{1.2.3 \times 1.2.3 \dots m-3} \times \frac{1}{a-\beta-3 \times a-\beta-2 \times a-\beta-1, a-\beta+n-3 \times \gamma-} \\
 & \frac{1}{\beta-3, \gamma-\beta-2, \gamma-\beta-1, \gamma-\beta+r-3 \times \delta-\beta-3, \delta-\beta+s-3 \times \&c.} \dots \\
 & \frac{1}{1.2.3.4 \dots m} \times \frac{1}{a-\beta-m, a-\beta-m+1, a-\beta+n-m \times \gamma-\beta-m, \gamma-\beta-} \\
 & \frac{1}{m+1, \gamma-\beta-m+r \times \delta-\beta-m, \delta-\beta-m+1, \delta-\beta-m+s \times \&c.} = 0; \\
 & \frac{1}{1.2.3 \dots r} \times \frac{1}{a-\gamma, a-\gamma+1, a-\gamma+n \times \beta-\gamma, \beta-\gamma+1, \beta-\gamma+m} \\
 & \frac{1}{\times \delta-\gamma, \delta-\gamma+1, \delta-\gamma+s \times \&c.} - \&c. = 0, \&c. = 0; \text{ or, to explain}
 \end{aligned}$$

it otherwise, assume the quantities $a, a+1, a+2, a+3, \dots a+n$; $\beta, \beta+1, \beta+2, \dots \beta+m$; $\gamma, \gamma+1, \gamma+2, \dots \gamma+r$; $\delta, \delta+1, \delta+2, \dots \delta+s$; &c. subtract one a from all the remaining quantities $a+1, a+2, \dots a+n$; $\beta, \beta+1, \dots \beta+m$; $\gamma, \gamma+1, \dots \gamma+r$; $\delta, \delta+1, \dots \delta+s$; &c. and multiply all the differences resulting $1, 2, 3, \dots n$; $\beta-a, \beta-a+1, \dots \beta-a+m$; $\gamma-a, \gamma-a+1, \dots \gamma-a+r$; $\delta-a, \delta-a+1, \dots \delta-a+s$, &c. into each other, and call the content p . In the same manner subtract $a+1$ from all the remaining quantities $a, a+2, a+3, \dots a+n$; $\beta, \beta+1, \dots \beta+m$; $\gamma, \gamma+1, \dots \gamma+r$; $\delta, \delta+1, \dots \delta+s$; &c.; and let the remainders $-1, 1, 2, 3, \dots n-1$; $\beta-a-1, \beta-a, \beta-a+1, \dots \beta-a+m-1$; $\gamma-a-1, \gamma-a, \gamma-a+1, \dots \gamma-a+r-1$; $\delta-a-1, \delta-a, \delta-a+1, \dots \delta-a+s-1$, &c. be multiplied together, and their content be called p^{+1} . In the same manner subtract $a+2, a+3, \dots a+n$ respectively from all the remaining quantities, and let the differences re-

sulting

sulting be multiplied together, and their respective contents be called $p^{+1}, p^{+2}, p^{+3}, \dots p^{+n}$; then, if the sum of the series can be found, will $\frac{1}{p^a} + \frac{1}{p^{a+1}} + \frac{1}{p^{a+2}} + \frac{1}{p^{a+3}} \dots \frac{1}{p^{a+n}} = 0$.

In the same manner subtract $\beta, \beta+1, \beta+2, \dots \beta+m$ respectively from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^{\beta}, p^{\beta+1}, p^{\beta+2}, \dots p^{\beta+m}$, then will $\frac{1}{p^{\beta}} + \frac{1}{p^{\beta+1}} + \frac{1}{p^{\beta+2}} \dots \frac{1}{p^{\beta+m}} = 0$.

Subtract $\gamma, \gamma+1, \gamma+2, \dots \gamma+r$ respectively from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^{\gamma}, p^{\gamma+1}, p^{\gamma+2}, \dots p^{\gamma+r}$; then will $\frac{1}{p^{\gamma}} + \frac{1}{p^{\gamma+1}} + \frac{1}{p^{\gamma+2}} \dots \frac{1}{p^{\gamma+r}} = 0$.

Subtract $\delta, \delta+1, \delta+2, \dots \delta+s$ from all the remaining quantities, and multiply their respective remainders into each other, and call their contents respectively $p^{\delta}, p^{\delta+1}, p^{\delta+2}, p^{\delta+3}$; then will $\frac{1}{p^{\delta}} + \frac{1}{p^{\delta+1}} + \frac{1}{p^{\delta+2}} + \frac{1}{p^{\delta+3}} = 0$; and so on; and, *vice versa*, if the sum of the above-mentioned fractions be respectively $= 0$; then the sum of the series, whose general term is the given one, can be found; otherwise not.

13. If the sum of the series, whose general term is

$$\frac{1}{x+a \cdot x+a+1 \dots x+a+n} x = +\beta \cdot x+\beta+1 \dots x+\beta+m \times x+\gamma \cdot x+\gamma+1 \dots$$

$$x+\gamma+r \times x+\delta \cdot x+\delta+1 \dots x+\delta+s \times &c. = H,$$

can be found in finite terms; then the sum of a series, whose general term is

$$\frac{x^l + bx^{l-1} + cx^{l-2} + &c.}{H} \quad (\text{where } l \text{ denotes an affirmative number})$$

can be also expressed in finite terms.

14. Let

14. Let $A \times \overline{z+\beta} \cdot \overline{z+\gamma} \cdot \overline{z+\delta} \times \overline{z+\epsilon} \times \&c. + B \times \overline{z+\alpha} \times \overline{z+\gamma} \cdot \overline{z+\delta} \cdot \overline{z+\epsilon} \cdot \&c. + C \times \overline{z+\alpha} \cdot \overline{z+\beta} \times \overline{z+\delta} \cdot \overline{z+\epsilon} \cdot \&c. + D \times \overline{z+\alpha} \cdot \overline{z+\beta} \cdot \overline{z+\gamma} \times \overline{z+\epsilon} \cdot \&c. + E \times \overline{z+\alpha} \cdot \overline{z+\beta} \cdot \overline{z+\gamma} \cdot \overline{z+\delta} \times \&c. = 1$, whatever may be the value of z ; then will

$$A = \frac{1}{\beta-\alpha, \gamma-\alpha, \delta-\alpha, \&c.}, B = \frac{1}{\alpha-\beta, \gamma-\beta, \delta-\beta, \&c.}, C = \frac{1}{\alpha-\gamma, \beta-\gamma, \delta-\gamma, \&c.},$$

$$D = \frac{1}{\alpha-\delta, \beta-\delta, \gamma-\delta, \&c.}, \&c.$$

15. Let the general term of a series be $\frac{1}{H} \times e^x$, where e is less than 1; the sum of the series can be expressed in finite

terms, when the above-mentioned quantities $\frac{1}{p\alpha} + \frac{1}{p\alpha+1} + \frac{1}{p\alpha+2}$

$$+ \frac{1}{p\alpha+3} \dots + \frac{1}{p\alpha+n} = 0, \frac{1}{p\beta} + \frac{1}{p\beta+1} + \frac{1}{p\beta+2} + \frac{1}{p\beta+3} \dots + \frac{1}{p\beta+m} = 0,$$

$$\frac{1}{p\gamma} + \frac{1}{p\gamma+1} + \frac{1}{p\gamma+2} \dots + \frac{1}{p\gamma+r} = 0, \frac{1}{p\delta} + \frac{1}{p\delta+1} + \frac{1}{p\delta+2} + \frac{1}{p\delta+3} \dots$$

$$+ \frac{1}{p\delta+s} = 0, \&c. = 0. \text{ This never happens unless } e = 1.$$

16. If the general term be any rational function of z into the exponential e^x , viz. $\frac{ax^l + bx^{l-1} + cx^{l-2} + \&c.}{z+\alpha \times z+\alpha+\beta \times z+\alpha+\gamma \times \&c. \times z+\beta \times$

$$\frac{z+\beta+\beta' \times z+\beta+k' \times \&c. \times z+\gamma \times z+\gamma+b'' \times z+\gamma+k'' \times \&c.}{\times e^x} = K,$$

where $h, b, b', \&c. k, k', k', \&c. l, m, m', m', \&c. n, n', n', \&c. r, r', r', \&c. \&c.$ denote whole numbers, and neither

$\alpha - \beta$, nor $\alpha - \gamma$, nor $\beta - \gamma$, &c. are whole numbers: let m be the greatest of the indices $m, m', m'',$ &c.; n the greatest of the indices $n, n', n'',$ &c.; r the greatest of the indices $r, r', r'',$ &c.; then, from the sums of the serieses given, whose general terms are $\frac{z^{\alpha}}{(z+\alpha)^m}, \frac{z^{\beta}}{(z+\alpha)^{m-1}}, \frac{z^{\gamma}}{(z+\alpha)^{m-2}}, \dots, \frac{z^{\alpha}}{(z+\alpha)^2}, \frac{z^{\alpha}}{z+\alpha},$
 $\frac{z^{\beta}}{(z+\beta)^n}, \frac{z^{\beta}}{(z+\beta)^{n-1}}, \dots, \frac{z^{\beta}}{z+\beta}, \frac{z^{\gamma}}{(z+\gamma)^r}, \frac{z^{\gamma}}{(z+\gamma)^{r-1}}, \dots, \frac{z^{\gamma}}{z+\gamma},$ &c. can be deduced the sum of the above series, whose general term is given above; multiply each of these terms into unknown coefficients $e', f, g, h,$ &c.; then reduce them to a common denominator, which is the same as the denominator of the given general term, and add them together, and make the correspondent terms of the sum resulting equal to the correspondent terms of the numerator $az^l + bz^{l-1} + cz^{l-2} + \&c.$ of the given general term; and from the equations resulting can be deduced the co-efficients $e', f, g, h,$ &c. and thence from the given sums the sum of the series required.

Approximations to the sums of the serieses may be deduced from the methods given in the *Meditationes Analyticae*. The sum of some few cases have been given from the periphery of the circle: for example, when α and m are whole numbers, and $e=1$; or, more particularly, when $m=2$ and $e=\frac{1}{2}$, and some other particular cases, which may be with nearly the same facility calculated from approximations; the cases given indeed are so few, unless when $e=1$, that they can very rarely be applied.

17. If the dimensions of z in the numerator be equal or greater than its dimensions in the denominator; that is, l be equal or greater than $m+m'+m''+\&c. + n+n'+n''+\&c. + r+r'+r''$

+ &c. &c. reduce the fractions to a mixed number, so that the dimensions of z in the numerator of the fraction be less than its dimensions in the denominator, and the integral part be $Ae^z + Bze^z + Cz^2e^z + Dz^3e^z + \dots Hze^z$; the sum of the infinite series whose term is $Ae^z + Bze^z + Cz^2e^z + Dz^3e^z + Ez^4e^z + \dots$

$+ Hze^z = \frac{Ae}{1-e} + B\left(\frac{1}{(1-e)^2} - \frac{1}{1-e}\right) + C\left(\frac{1 \cdot 2}{(1-e)^3} - \frac{1}{(1-e)^2}\right) + D\left(\frac{1 \cdot 2 \cdot 3}{(1-e)^4} - \frac{1+2 \times 1 \cdot 2}{(1-e)^3} + \frac{1}{(1-e)^2}\right) + E\left(\frac{1 \cdot 2 \cdot 3 \cdot 4}{(1-e)^5} - \&c.\right)$: the sum of a series (whose general term is $z^m e^z$) = $\frac{1 \cdot 2 \cdot 3 \cdot 4 \dots m}{(1-e)^{m+1}} e -$

$\frac{1 \cdot 2 \dots m-1}{(1-e)^m} 1 \cdot 2 \cdot 3 \dots m-1 e + \frac{1 \cdot 2 \dots m-2}{(1-e)^{m-1}} \times 1 \cdot 2 \cdot 3 \dots m-2 e$

$\dots + \frac{L}{(1-e)^2} \times 1 \cdot 2 \cdot 3 \dots m-b-1 \dots = \frac{1}{(1-e)^2}$, where L is

equal to the sum of all quantities of the following sort,

$\cdot S^{\alpha-1} \times S^{\beta-1} \times S^{\gamma-1} \times S^{\delta-1} \times S^{\epsilon-1} \times \&c.$

where $\alpha, \beta, \gamma, \delta, \&c.$ are whole affirmative numbers; (in the preceding notation by $\cdot S$ is designed the sum of the contents of every π of the following numbers 1, 2, 3, 4, 5, $\dots \ell$); and $\alpha + \beta + \gamma + \delta + \dots = b + 1$; the above-mentioned product $\cdot S^{\alpha-1} \times \cdot S^{\beta-1} \times \&c.$ is to be taken affirmative or negative, according as the number of letters $\alpha, \beta, \gamma, \delta, \&c.$ is even or uneven.

The sum of the series $z^m \times e^z$ may also be found by assuming for it $(az^m + bz^{m-1} + cz^{m-2} + dz^{m-3} \dots k)e^z$; then, finding its

successive term $(a \times z + 1 e + bz + 1 e + cz + 1 e + \&c.) e^z$, and taking the difference between it and the assumed quantity, there results $(a \times e - 1 z^m + mae + be - 1 z^{m-1} + \&c.) e^z$; by equating it to the given term $z^m e^z$ are deduced the subsequent equations $a \times e - 1 = 1$, $mae + be - 1 = 0$, &c. whence

$a = \frac{1}{e-1}$, $b = \frac{1}{e} = ma$, &c.

If $e = 1$, then assume $ax^{n+1} + bx^n + \&c.$ for the sum sought, which rule was first taught by M. J. BERNOULLI.

If the term be $x^n f^{ls}$; for f^{ls} substitute e , and there results $x^n e^n$ the same as before.

18. Let $P = A + Bx^n + Cx^{2n} + Dx^{3n} + \&c.$ then will the sum of the series $\frac{A}{a \cdot \beta \cdot \gamma \cdot \delta \cdot \&c.} + \frac{Bx^n}{a+n \cdot \beta+n \cdot \gamma+n \cdot \delta \cdot \&c.} +$

$$\frac{Cx^{2n}}{a+2n \cdot \beta+2n \cdot \gamma+2n \cdot \delta \cdot \&c.} + \&c. = \frac{1}{a\beta\gamma\delta \cdot \&c.} \times P - \frac{1}{a} \cdot \frac{1}{\beta-a} \cdot \frac{1}{\gamma-a} \cdot \frac{1}{\delta-a} \cdot \&c. \times \&c. x^{-n} \int x^n \dot{p} - \frac{1}{\beta} \cdot \frac{1}{a-\beta} \cdot \frac{1}{\gamma-\beta} \cdot \frac{1}{\delta-\beta} \cdot \&c. x^{-2n} \int x^{2n} \dot{p} - \frac{1}{\gamma} \cdot \frac{1}{a-\gamma} \cdot \frac{1}{\beta-\gamma} \cdot \frac{1}{\delta-\gamma} \cdot \&c. \times x^{-3n} \int x^{3n} \dot{p} - \&c.$$

This may be proved from the subsequent arithmetical proposition $\frac{1}{a} \cdot \frac{1}{\beta-a} \cdot \frac{1}{\gamma-a} \cdot \frac{1}{\delta-a} \cdot \&c. + \frac{1}{\beta} \cdot \frac{1}{a-\beta} \cdot \frac{1}{\gamma-\beta} \cdot \frac{1}{\delta-\beta} \cdot \&c. + \frac{1}{\gamma} \cdot \frac{1}{a-\gamma} \cdot \frac{1}{\beta-\gamma} \cdot \frac{1}{\delta-\gamma} \cdot \&c. + \frac{1}{\delta} \cdot \frac{1}{a-\delta} \cdot \frac{1}{\beta-\delta} \cdot \frac{1}{\gamma-\delta} \cdot \&c. = \frac{1}{a \cdot \beta \cdot \gamma \cdot \delta \cdot \&c.}$

19. Let the general term of the above-mentioned series $A + Bx^n + Cx^{2n} + \&c.$ be Hx^{ln} ; then from the sums of the series p , and the fluents of the fluxions $x^n \dot{p}$, $x^{2n} \dot{p}$, $x^{3n} \dot{p}$, $x^{4n} \dot{p}$, &c. being given there follows the sum of a series, whose general term is $\frac{ax^{ln} + bx^{l-1n} + cx^{l-2n} + \&c.}{a+n \cdot \beta+n \cdot \gamma+n \cdot \delta \cdot \&c.} \times Hx^{ln}$, where l denotes a whole number.

If $H = m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \dots \cdot \frac{m-l}{l+1}$, or $= m \cdot \frac{m-1}{2} \cdot \frac{m-2}{3} \cdot \dots \cdot \frac{m-l}{l+1}$, where l is a whole number, and $m, a', \beta', \gamma', \&c.$ are either whole numbers or fractions whose denominator is 2, and $a = a'n, \beta = \beta'n, \&c.$ the sum of the above-mentioned series can be found by finite terms, circular arcs and logarithms. If

If $H = \frac{1}{1 \cdot 2 \cdot 3 \dots x}$ or $= \frac{1}{1 \cdot 2 \cdot 3 \dots k}$, and h, m, p, y , &c. whole numbers and $n = 1$; then can the sum of any series of the above-mentioned formula be found in finite algebraical functions of x , and the circular arcs and logarithms of them.

PART II

P. THE doctrine of proportional parts was probably very early known in the æra of science; for when men could not find the exact value of a quantity, they were induced to find near approximations by trials, and from thence, by proportion, an approximation still nearer: which method is commonly denominated the Rule of False.

This was often found to deviate considerably from the exact value; and the same operation was repeated, which frequently produced a nearer approximate value, and so on.

This method of approximations, the most general yet known, has been used in resolving problems by several of the most eminent mathematicians in different ages, and in this particularly by M. EULER.

1. The following observation, I believe, was first published in the *Meditationes*, in the year 1770, viz. that the convergency of the approximate values, found by the rule of false and method of infinite series, generally depended on this, viz.

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how much nearer the approximate assumed is to one value of the quantity sought possible or impossible than to any other, and not to the quantity itself: hence, when two or more (n) values of the quantity sought are nearly equal, it is necessary to recur to more difficult rules, *viz.* to three or more trials; as, for example, let two roots be nearly equal, and write a , $a + \pi$, and $a + \rho$, for the unknown quantity in the given equation made $= 0$, and let the quantities resulting be A , B , and C , then will more near approximations to the two roots nearly equal of the given equation be $a +$ the two roots (x) of the quadratic $\left(\frac{A}{\pi\rho} + \frac{B}{\pi(\pi-\rho)} + \frac{C}{\rho(\rho-\pi)}\right)x^2 + \left(A \times \frac{\pi+\rho}{\pi\rho} + B \times \frac{\rho}{\pi \cdot (\pi-\rho)} + \frac{C\pi}{\rho \cdot (\rho-\pi)}\right)x + A = 0$: for write σ , π , and ρ , respectively for x in the equation, and there will result the quantities A , B , and C .

More generally, substitute for x in the given equation the quantities a , $a + \pi$, $a + \rho$, $a + \sigma$, $a + \tau$, $a + \nu$, &c. where π , ρ , σ , τ , &c. are very small quantities; and let the quantities resulting be A , B , C , D , E , F , &c.; then will more near approximations to the (n) roots of the given equation be $a + \alpha$, $a + \beta$, $a + \gamma$, $a + \delta$, &c. where α , β , γ , δ , &c. are the n roots (e) of the given equation $\frac{(e-\pi)(e-\rho)(e-\sigma)(e-\tau)\&c.}{\pm \pi\rho\sigma\tau\&c.} \times A + \frac{e(e-\rho)(e-\sigma)(e-\tau)\&c.}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)\&c.}$

$$\begin{aligned} & \times B + \frac{e(e-\pi)(e-\sigma)(e-\tau)\&c.}{e(e-\pi)(e-\sigma)(e-\tau)\&c.} \times C + \&c. = A - \left(\frac{A}{\pi} - \frac{B}{\pi}\right)e + \left(\frac{A}{\pi\rho} + \frac{B}{\pi(\pi-\rho)} + \frac{C}{\rho(\rho-\pi)}\right) \\ & \times e \cdot e - \pi - \left(\frac{A}{\pi\rho\sigma} + \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)} + \frac{C}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)\&c.} + \frac{D}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\tau)\&c.} + \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)\&c.}\right) \\ & \times e \cdot e - \pi \cdot e - \rho + \left(\frac{A}{\pi\rho\sigma\tau} + \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)} + \frac{C}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\tau)\&c.} + \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)\&c.} + \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)\&c.}\right) \\ & \times e \cdot e - \pi \cdot e - \rho \cdot e - \sigma - \left(\frac{A}{\pi\rho\sigma\nu} + \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)(\pi-\nu)} + \frac{C}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\tau)(\rho-\nu)} + \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)(\sigma-\nu)} + \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)(\tau-\nu)} + \frac{F}{\nu(\nu-\pi)(\nu-\rho)(\nu-\sigma)(\nu-\tau)\&c.}\right) \\ & \times e \cdot e - \pi \cdot e - \rho \cdot e - \sigma \cdot e - \nu - \left(\frac{A}{\pi\rho\sigma\nu\&c.} + \frac{B}{\pi(\pi-\rho)(\pi-\sigma)(\pi-\tau)(\pi-\nu)\&c.} + \frac{C}{\rho(\rho-\pi)(\rho-\sigma)(\rho-\tau)(\rho-\nu)\&c.} + \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)(\sigma-\nu)\&c.} + \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)(\tau-\nu)\&c.} + \frac{F}{\nu(\nu-\pi)(\nu-\rho)(\nu-\sigma)(\nu-\tau)\&c.} + \frac{G}{\&c.(\&c.-\pi)(\&c.-\rho)(\&c.-\sigma)(\&c.-\tau)(\&c.-\nu)\&c.}\right) \\ & \times e \cdot e - \pi \cdot e - \rho \cdot e - \sigma \cdot e - \nu \cdot e - \&c. - \&c. \end{aligned}$$

(e-7)

$$\frac{(e-\tau)(e-v)}{v(v-\pi)(v-\xi)(v-\sigma)(v-\tau)} = \frac{D}{\sigma(\sigma-\pi)(\sigma-\xi)(\sigma-\tau)(\sigma-v)} = \frac{E}{\tau(\tau-\pi)(\tau-\xi)(\tau-\sigma)(\tau-v)} = \frac{F}{\dots} \dots e \cdot e - \pi \cdot e - \rho \cdot e - \sigma \cdot e - \tau + \&c. = \alpha.$$

Their resolutions were first given in the *Meditationes Analyticae*, published in the year 1774, and require the extraction of a quadratic, cubic, and in general of an equation of (n) dimentionis; which rules will often give a nearer approximate than the preceding, when the roots are not nearly equal.

3. These rules may be applied to find approximations to the roots of algebraical equations: for example, let the algebraical equation be $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$, substitute in it for x two quantities a , and $a + e$ much nearer to one root than to any other, and there result $a^n - pa^{n-1} + qa^{n-2} - \&c. = A$, and $(a + e)^n - p(a + e)^{n-1} + q(a + e)^{n-2} - \&c. = B$; then, by the rule of

false, $B - A : e :: A : \frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{(na^{n-1} - n - 1pa^{n-2} + n - 2qa^{n-3} - \&c.) + \&c.} = b$;

whence $a - b$ a near approximate value to the root sought. If the quantities, in which are involved $e, e^2, e^3, \&c.$ on account of e being very small, be rejected, then will the approximate sought $b =$

$\frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{na^{n-1} - n - 1pa^{n-2} + n - 2qa^{n-3} - \&c.}$; which will nearly be the same

as found, where a near approximate is given, from the method given by VIETA, HARRIOT, OUGHTRED, NEWTON, DE LAGNY, HALLEY, &c.

4. From this expression it follows, that if (a) be a root of the equation $na^{n-1} - n - 1pa^{n-2} + \&c. = 0$, of which the roots are limits between the roots of the given equation, the approximation found will be infinite.

5. In finding these approximations, when there are two or more quantities contained in the given equation dependent on each

each other, as the arc and the sine, it is necessary that both should be corrected in every approximation to such a degree as the subsequent approximations require.

6. In the *Meditations* it is observed, that in any algebraical equation $x^m = ax^{m-1} + bx^{m-2} + cx^{m-3} + dx^{m-4} + ex^{m-5} \dots \pm gx^{m-n+1} \pm bx^{m-n} \pm kx^{m-n-1} \pm lx^{m-n-2} \pm \&c. = 0$, if a be much greater than $\frac{b}{a}$, and $\frac{b}{a}$ has to $\frac{c}{b}$ a much greater ratio than $a : \frac{b}{a}$; and in the same manner $\frac{c}{b}$ has to $\frac{d}{c}$ a much greater ratio than $\frac{b}{a} : \frac{c}{b}$, and so on; then will a be a near approximate to the greatest root of the algebraical equation; $\frac{b}{a}$ a near approximate to the second; $\frac{c}{b}$ a near approximate to the third, and $\frac{d}{b}$ a near approximate to a root, which is much less than m roots of the given equation, but much greater than the remaining $(n - m - 1)$ roots.

If the equation above-mentioned $x^m \pm ax^{m-1} + \&c. = 0$, or which is the same, $1 \pm \frac{a}{x} + \frac{b}{x^2} \pm \&c. = 0$ be infinite; then will, in like manner, all its roots be possible and their approximate values $a, \frac{b}{a}, \frac{c}{b}, \&c.$ as before.

This easily appears by substituting for $a, b, c, \&c.$ their values in terms of the root of the equation.

7. A nearer approximate to the above-mentioned root will be $\frac{k}{b} - \left(\frac{1}{k} - \frac{gk^2}{b^3} \right) + \&c.$

8. Equations, of which the fluxions of the quantities contained in the given equations can be found, may be reduced to infinite algebraical equations, in which is involved no irrational function of the unknown quantities contained in the given equations by the incremental theorem; viz. let $A = 0$ be the given equation,

equation, and (a) an approximate much more near to the root required (π) of the given equation than to any other: write a for (x) the unknown quantity sought in the subsequent quantities, $A, \frac{A}{x}, \frac{A}{x^2}, \frac{A}{x^3}, \&c.$; and let there result the correspondent quantities $\alpha, \beta, \gamma, \delta, \&c.$; then will $\pi - a$ be a root (e) of the infinite equation $\alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 \pm \frac{1}{1 \cdot 2 \cdot 3} \delta e^3 + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} \epsilon e^4 \pm \&c. = 0$, of which a root of the equations $\alpha \pm \beta e = 0, \alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 = 0, \&c.$ will be an approximate. If two roots of the given equation are nearly $= a$, then it is necessary to recur to an equation not inferior to a quadratic.

9. The successive approximate values found by these and like rules will ultimately be to each other in a greater than any geometrical ratio: for example, let $\frac{as}{a+1}$ be an approximate to a root of the quadratic $x^2 - (a+1)x + as = 0$, then will the new addition to the approximate to the root s found by the common method at the distance $\pi - 1$ from the first approximate be $\frac{s^b}{a^{\pi-1}}$ nearly, where $b = 2^{\pi-1}$.

10. Let an equation $x^n - Px^{n-1} + Qx^{n-2} - Rx^{n-3} + Sx^{n-4} - \&c. = 0$, of which the roots are $a, b, c, d, \&c.$; and an equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} - \&c. = 0$, where $p, q, r, s, \&c.$ differ from the co-efficients $P, Q, R, S, \&c.$ by very small quantities: assume the (n) equations $\pi + \rho + \sigma + \tau + \&c. = p - P = a, a\pi + b\rho + c\sigma + d\tau + \&c. = Pa - q + Q = \beta, a^2\pi + b^2\rho + c^2\sigma + d^2\tau + \&c. = P\beta - Qa + r - R = \gamma, a^3\pi + b^3\rho + c^3\sigma + d^3\tau + \&c. = P\gamma - Q\beta + Ra - s + S = \delta, \&c.$; and from them find the unknown quantities $\pi, \rho, \sigma, \tau, \&c.$; then will $a + \pi, b + \rho, c + \sigma, d + \tau, \&c.$ be nearly the n roots of the equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} - \&c. = 0$.

- &c. = 0, and consequently $\pi, p, \&c.$ nearly $-\frac{a^2 - p^2 - b^2 + q^2 - r^2 - \&c.}{2a - 2p - 2b + 2q - 2r + \&c.}$,
 $\frac{b^2 - p^2 - 1 + q^2 - r^2 - \&c.}{nb^{n-1} - n - 1 p^{n-1} + \&c.}$, &c.; whence the convergency of the approximate values found by this rule depends on the principle before delivered.

10. Let there be given (m) equations, which contain m unknown quantities $x, y, z, \&c.$; and let $\alpha, \beta, \gamma, \&c.$ be nearly correspondent values of the unknown quantities $x, y, \&c.$ respectively: assume $n + 1$ different values of the quantity x , viz. $\alpha, \alpha + \pi, \alpha + \pi', \alpha + \pi'', \&c. \&c.$; and in like manner assume $n + 1$ different correspondent values of the quantity y , which let be $\beta, \beta + p, \beta + p', \beta + p'', \&c.$; and so of the remaining; where $\pi, \pi', \pi'', \&c. p, p', p'', \&c. \&c.$ are very small quantities; substitute these quantities for their respective values in the given equations, and let the resulting quantities be $A, B, C, D, \&c.$ in the first equation; $P, Q, R, S, \&c.$ in the second, &c. &c.: assume from the first equation the n simple equations $a\pi + bp + \&c. = B - A, a\pi' + bp' + \&c. = C - A, a\pi'' + bp'' + \&c. = D - A, \&c.$; and from the second equation the n simple ones $b\pi + kp + \&c. = Q - P, b\pi' + kp' + \&c. = R - P, b\pi'' + kp'' + \&c. = S - P, \&c.$ From these equations can be investigated the co-efficients $a, b, \&c. h, k, \&c. \&c.$; ultimately assume the m equations $A + ae + bi + \&c. = 0, P + be + ki + \&c. = 0, \&c.$ from which can be deduced the values of the quantities $e, i, \&c.$; and $\alpha + e, \beta + i, \&c.$ will be more near values of the quantities, $x, y, \&c.$

11. Sir ISAAC NEWTON found the sum (A) of the $2n^{\text{th}}$ power of each of the roots of a given equation, and then extracted the $2n^{\text{th}}$ root of A , viz. $\sqrt[2n]{A}$ for an approximate value of the greatest root of the equation, and further added some similar rules on the same principle. In

In the *Miscell. Analyt. and Meditations* the same principle is applied in different rules for finding approximates to the greatest and other roots of the given equation; and also limits of the ratios of the approximate values of the roots found by these rules to the roots themselves are given.

It is observed in the *Meditations*, that from these rules in general to find the greatest root, it is often necessary that the greatest possible root be greater than the sum of the quantities contained in the possible and impossible part of any impossible root of the given equation: for example, if $a+b\sqrt{-1}$ be an impossible root of the given equation, then it is necessary that the greatest possible root be greater than $a+b$.

It may further be observed, that in equations of high dimensions (unless purposely made) it is probable, the number of impossible will greatly exceed the number of possible roots; and consequently these rules most commonly fail.

12. M. BERNOULLI assumed a fraction whose numerator is a rational function of the unknown quantity, and denominator the quantity, which constitutes the equation; and reduced the fraction into a series, whose terms proceed according to the dimensions of the unknown quantity; and thence found an approximate value of the greatest or least root of the given equation or its reciprocal, by dividing the co-efficient of any term of the series resulting by the co-efficient of the preceding or subsequent term: for example, let the equation be

$$x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} \dots \pm Px^2 \mp Qx \mp S = 0; \text{ as-}$$

$$\text{sume the fraction } \frac{nx^{n-1} - (n-1)px^{n-2} + (n-2)qx^{n-3} - (n-3)rx^{n-4} + \&c.}{x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + \&c.}$$

$$= nx^{n-1} + (a + \beta + \gamma + \delta + \&c.)x^{n-2} + (a^2 + \beta^2 + \gamma^2 + \delta^2 + \&c.)x^{n-3} + (a^3 + \beta^3 + \gamma^3 + \delta^3 + \&c.)x^{n-4} + (a^4 + \beta^4 + \gamma^4 + \delta^4 + \&c.)$$

$\&c.)x^{-5} \dots + (\alpha^{n-1} + \beta^{n-1} + \gamma^{n-1} + \&c. = P)x^{-n} + (\alpha^n + \beta^n + \gamma^n + \&c. = Q)x^{-n-1} + (\alpha^{n+1} + \beta^{n+1} + \gamma^{n+1} + \&c. = R)x^{-n-2} + \&c.$; then will $\frac{R}{Q}$ or $\sqrt[n]{Q}$ be the greatest root nearly.

Ex. 2. $\frac{R - 2Qx + 3Px^2 - \&c. \dots nx^{n-1}}{S - Rx + Qx^2 - Px^3 + \&c. \dots x^n} = \left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} + \&c. \right) + \left(\frac{1}{\alpha^2} + \frac{1}{\beta^2} + \frac{1}{\gamma^2} + \frac{1}{\delta^2} + \&c. \right)x + \left(\frac{1}{\alpha^3} + \frac{1}{\beta^3} + \frac{1}{\gamma^3} + \&c. \right)x^2 + \left(\frac{1}{\alpha^4} + \frac{1}{\beta^4} + \frac{1}{\gamma^4} + \&c. \right)x^3 + \dots + \left(\frac{1}{\alpha^{n-1}} + \frac{1}{\beta^{n-1}} + \frac{1}{\gamma^{n-1}} + \&c. = P \right)x^{n-2} + \left(\frac{1}{\alpha^n} + \frac{1}{\beta^n} + \frac{1}{\gamma^n} + \&c. = Q \right)x^{n-1} + \left(\frac{1}{\alpha^{n+1}} + \frac{1}{\beta^{n+1}} + \frac{1}{\gamma^{n+1}} + \&c. \right)x^n + \&c.$; then will $\frac{P}{Q}$ or $\sqrt[n]{\frac{P}{Q}}$, the least root nearly.

Ex. 3. $\frac{1}{x^n - px^{n-1} + qx^{n-2} - \&c.} = x^{-n} + (\alpha + \beta + \gamma + \delta + \&c.)x^{-n-1} + (\alpha^2 + \beta^2 + \gamma^2 + \&c. + \alpha\beta + \alpha\gamma + \beta\gamma + \alpha\delta + \&c.)x^{-n-2} + (\alpha^3 + \beta^3 + \gamma^3 + \&c. + \alpha^2\beta + \alpha^2\gamma + \beta^2\gamma + \gamma^2\alpha + \gamma^2\beta + \&c. + \alpha\beta\gamma + \alpha\beta\delta + \alpha\gamma\delta + \beta\gamma\delta + \&c.)x^{-n-3} + \&c.$; and $\frac{S}{S - Rx + Qx^2 - \&c. \dots x^n} = 1 + \left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} + \&c. \right)x + \left(\frac{1}{\alpha^2} + \frac{1}{\beta^2} + \frac{1}{\gamma^2} + \&c. + \frac{1}{\alpha\beta} + \frac{1}{\alpha\gamma} + \frac{1}{\beta\gamma} + \&c. \right)x^2 + \&c.$ in each of which all the numeral co-efficients are 1. The approximate values to the greatest and least root may be found in the same manner as before.

From the preceding examples it appears, that the same observations which have been applied to Sir ISAAC NEWTON's method are equally applicable to M. BERNOULLI's.

13. In the *Meditationes* this rule is further extended, viz. let the given equation involve irrational and other functions of the unknown quantity; reduce it so that no function of the unknown quantity (x) may be contained in the denominator, and let the resulting equation be $A = 0$. Assume a fraction

$$\frac{B}{A},$$

$\frac{B}{A}$, whose numerator B is a finite rational and integral function of the unknown quantity; reduce $\frac{B}{A}$ into a series proceeding according to the dimensions of the unknown quantity: for example, let the series be $A'x^r + B'x^{r+1} + Cx^{r+2} + \dots + Lx^{r+n} + Mx^{r+n+1} + Nx^{r+n+2} + \&c.$; then (*exceptis excipiendis*) if s be negative, will the greatest root be $\sqrt[r]{\frac{M}{L}}$ nearly; but, if s be affirmative, $\sqrt[r]{\frac{L}{M}}$ will be the least root nearly. If s be infinite, then (*exceptis excipiendis*, as before-mentioned) the quantities $\sqrt[r]{\frac{M}{L}}$ and $\sqrt[r]{\frac{L}{M}}$ will be the above-mentioned roots accurately.

These principles have been applied to find the remaining roots of the given equation as well as the greatest and least.

14. The rule of false has been found very useful in finding approximates to the two unknown quantities contained in two given equations, and has been applied to (n) equations having (n) different unknown quantities: for example, it has been observed, that if two or more (m) values of an unknown quantity (x) are nearly equal to each other and to its given approximate value (x'), the unknown quantity $v = x - x'$ will ascend to two or more (m) dimensions in one of the resulting equations; or in more than one equations will be contained such powers of the quantity (v), that if the more equations were reduced to one whose unknown quantity is v , the resulting equation will contain (m) dimensions of the quantity v . Hence it appears, that in this case also the convergency of the approximate values found will depend on the given approximate being much more near to one root than to any other.

15. When

15. When the given equations $A=0$, $B=0$, $C=0$, &c. contain irrational or other quantities whose fluxions can be found; and approximates $(a, b, c, d, \&c.)$ are given to each of the unknown quantities $(x, y, z, v, \&c.)$ contained in the given equations; let $a+x'=x$, $b+y'=y$, $c+z'=z$, $d+v'=v$, &c.: substitute in the quantities $A, \left(\frac{A'}{x}\right), \left(\frac{A'}{y}\right), \left(\frac{A'}{z}\right), \left(\frac{A'}{v}\right)$, &c.; $\left(\frac{\ddot{A}}{x^2}\right), \left(\frac{\ddot{A}}{xy}\right), \left(\frac{\ddot{A}}{xz}\right), \left(\frac{\ddot{A}}{xv}\right)$, &c.; $\left(\frac{\ddot{A}}{xyz}\right), \left(\frac{\ddot{A}}{xzy}\right)$, &c. for x, y, z, v , &c. respectively a, b, c, d , &c.; let the resulting correspondent values be $A', \left(\frac{A'}{x}\right), \left(\frac{A'}{y}\right)$, &c. whence may be deduced the equation $A' + \left(\left(\frac{A'}{x}\right) x' + \left(\frac{A'}{y}\right) y' + \left(\frac{A'}{z}\right) z' + \left(\frac{A'}{v}\right) v' + \&c.\right) + \left(\frac{1}{2} \left(\frac{A''}{x^2}\right) x'^2 + \frac{1}{2} \left(\frac{A''}{y^2}\right) y'^2 + \frac{1}{2} \left(\frac{A''}{x^2}\right) z'^2 + \&c. + \left(\frac{A'}{xz}\right) x' y' + \left(\frac{A''}{x^2 z}\right) x' z' + \&c.\right) + \&c. = 0$ in which no irrational, &c. function of the approximates x', y', z' , &c. is contained; and in the same manner may the remaining equations $B=0$, $C=0$, &c. be transformed into others, in which no irrational function of the approximates $(x', y', z', \&c.)$ is contained, and from the resulting equations may be found approximate values of the quantities $x', y', z', \&c.$

If there be given only two equations $A=0$ and $B=0$ containing two unknown quantities x and y , and all the quantities of the resulting equations, in which are contained more than one dimension of the quantities x' and y' be rejected, there will result the two equations $A' + \left(\frac{A'}{x}\right) x' + \left(\frac{A'}{y}\right) y' = 0$ and $B' + \left(\frac{B'}{x}\right) x' + \left(\frac{B'}{y}\right) y' = 0$, from which may be found x' and y' , the same

same is given by Mr. SIMPSON and others. When two or more values of the quantity x are nearly $= 0$, then in a resulting equation or equations, quantities of two or more dimensions of the approximate x are to be included.

P A R T III.

1. THE first algebraists divided quantities, and extracted their roots no further than the quantities themselves: they did not perceive the utility of proceeding any further, otherwise the operation would have been the same continued. Mr. GREGORY ST. VINCENT, and Mr. MERCATOR divided, and Sir ISAAC NEWTON divided and extracted the roots of quantities (in which only one unknown quantity (x) is contained) by the operations then used by arithmeticians, into series ascending or descending, according to the dimensions of x in *infinitum*. They clearly saw the utility of it in finding the fluents of fluxions, as Dr. WALLIS and others some little

time before had found the fluent of the fluxion $ax^{\frac{m}{n}}$; or, which is the same, the area of a curve whose ordinate is $ax^{\frac{m}{n}}$ and abscissa is x .

2. M. LEIBNITZ asked from Mr. NEWTON the cases in which the above-mentioned serieses would converge; for it would be altogether useless when it diverges, and of little use when it converges slowly.

To Case 1. To this question an answer, I believe, was first given in the *Meditationes*, viz. reduce the function to its lowest terms; and also in such a manner that the quantities contained in the numerator and denominator may have no denominator; make the denominator $Q=0$, and every distinct irrational quantity contained in it $=0$; and also every distinct irrational quantity H contained in the numerator $=0$; then, let α be the least root affirmative or negative (but not $=0$) of the above-mentioned resulting equations, the ascending series will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the above-mentioned series will not converge.

If the above-mentioned series (S) be multiplied into x , and its fluent found; then will the series denoting the fluent contained between two values a and b of the quantity (x) converge, when a and b are both contained between α and $-\alpha$: the fluent always converges faster than the series S, the unknown quantity x having the same values in both.

Ex. Let $\int \frac{x}{a+bx+cx^2+\dots x^n} = Ax + \frac{1}{2} Bx^2 + \frac{1}{3} Cx^3 + \&c.$ and the roots of the equation $a+bx+cx^2+\dots x^n=0$ be $\alpha, \beta, \gamma, \delta, \&c.$; then $\frac{x}{a+bx+cx^2+\dots x^n} = \frac{x}{a-x} + \frac{bx}{\beta-x} + \frac{cx^2}{\gamma-x} + \&c. = Ax + Bxx + Cx^2x + \&c.$; but $\frac{x}{a-x} = \frac{x}{a} + \frac{x^2}{a^2} + \frac{x^3}{a^3} + \&c.$ in infinitum, $\frac{cx^2}{\gamma-x} = \frac{cx^2}{\gamma} + \frac{cx^3}{\gamma^2} + \frac{cx^4}{\gamma^3} + \&c. \&c.$; the former series converges when x is contained between α and $-\alpha$, the latter when x is between β and $-\beta$, and so on. In the same manner the fluents $\int \frac{x^2}{a-x} = \frac{x^2}{a} + \frac{x^3}{2a^2} + \&c.$ $\int \frac{cx^2}{\beta-x} = \frac{cx^2}{\beta} + \frac{cx^3}{2\beta^2} + \&c. \&c.$ a fortiori converge when x is between α and $-\alpha$, β and $-\beta$, $\&c.$ respectively, and so on: hence the series $Ax + \frac{1}{2} Bx^2 + \frac{1}{3} Cx^3 + \&c.$

$$= \int \frac{x^k}{a+bx+cx^2 \dots x^n} = \int \frac{x^k}{a-x} + \int \frac{cx^k}{\beta-x} + \int \frac{cx^k}{\gamma-x} + \&c. = \left(\frac{x^k}{\alpha} + \frac{x^k}{\beta} + \frac{x^k}{\gamma} + \&c. \right) x + \left(\frac{x^k}{\alpha^2} + \frac{x^k}{\beta^2} + \frac{x^k}{\gamma^2} + \&c. \right) x^2 + \&c.$$
 always converges when x is between α and $-\alpha$, where α is the least root of the above-mentioned equation; but where x is greater than α or $-\alpha$, the series will diverge.

The infinite series $a^m + ma^{m-1}x + m \cdot \frac{m-1}{2} a^{m-2}x^2 + \&c. = \overline{a+x}^m$ will always converge when a is greater than x , and diverge when less, and consequently its convergency does not depend on the index m , unless when $x = \pm a$: and in the *Meditationes Analyticae* are given the cases in which it converges or diverges when $x = a$; and also if the series $x^m + max^{m-1} + \&c. = \overline{x+a}^m$ descends according to the dimensions of x , when it converges or diverges.

Caf. 2. Let the above-mentioned quantity be reduced into a series $Ax^r + Bx^{r-1} + \&c.$ descending according to the dimensions of the unknown quantity x ; then will the series $Ax^r + Bx^{r-1} + \&c. = P$, or the series $\frac{Ax^{r+1}}{1-r} - \frac{Bx^r}{r} + \&c. = \int P x$ converge, when x is greater than the greatest root (λ) of the above-mentioned equations, and diverge when it is less; and consequently in this case, when the fluent is required between the two values a and b of x ; the series found will converge when a and b are both greater than λ .

Caf. 3. When x is equal to the least root in the former case, and to the greatest in the latter, then sometimes the series will converge, and sometimes not. These different cases are given in the *Meditationes*; but it would be too long to insert them in this Paper.

4. If any roots are impossible as $a - b\sqrt{-1}$ and $a + b\sqrt{-1}$, then the series will converge when x is in the first case

less than $\pm(a-b)$, and in the second case greater than $\pm(a+b)$; or, more general, it will converge in the first case when x^n is always infinitely less than the reciprocal of the quantity $\frac{(a+b\sqrt{-1})^n + (a-b\sqrt{-1})^n}{(a^2+b^2)^n} = P$, where n is infinite; and in the latter case it will converge when x^n is infinitely greater than P .

It may not be improper to observe, that the same values of the root are to be applied in the equations, which are applied in the series.

3. Sir ISAAC NEWTON, in the binomial theorem, reduced the power or root of a binomial into a series proceeding according to the dimensions of the terms contained in the binomial. M. DE MOIVRE reduced the power or root of a multinomial into a like series; but in all cases, except the most simple, we must still recur to the common division, extraction of roots, &c.

4. Mess. EULER, MACLAURIN, and other mathematicians, finding that the serieses before-mentioned often converged slowly, or, if the truth may be confessed, commonly not at all, to deduce the area of a curve contained between two values a and b of the absciss, or fluent of a fluxion between two values a and b of the variable quantity x , interpolated the series or area between a and b ; that is, found the area or fluent contained between the abscissæ a and $a+\alpha$, then between the abscissæ $a+\alpha$ and $a+2\alpha$, and then between the abscissæ $a+2\alpha$ and $a+3\alpha$, and so on, till they came to the area between $b-\alpha$ and b . M. EULER observed, that when the ordinate became 0 or infinite, the series expressing the area converges slowly; and therefore,

in order to investigate the area near the points of the abscissæ, where the ordinates become 0 or infinite, he transforms the equation, and finds serieses expressing the area near those points, in which serieses the abscissæ or unknown quantities begin from those points.

5. In the *Meditationes* it is asserted, that in a series proceeding according to the dimensions of x , if any root of the above-mentioned equations be situated between the beginning of the abscissæ 0 and its end x , the series will not converge: it is therefore necessary to transform the abscissæ so that it may begin or end at each of the roots of the above-mentioned equations, and consequently where the ordinates become 0 or infinite, &c.; those cases excepted where the ordinate becomes 0, and its correspondent abscissæ is a root of a rational function W of x without a denominator, and $\int WPx$ is equal to the given series; and in general the abscissæ ought to begin from the above-mentioned points; for if they end there the series will converge very slow, if at all.

6. It is further asserted, that if a and b , the values of the abscissæ between which the area is required, be both more near to one root of the above-mentioned equations than to any other, and n serieses are to be found, whose sum express the area contained between a and b ; then that the sum of the (n) serieses may converge the swiftest, the distances of the beginnings of each of the n abscissæ from the adjacent root will form a geometrical progression.

7. Mr. CRAIG found the fluent of any fluxion of the formula $(a + bx^n + cx^{2n} + \&c.)^{-1} x^{n-1} x$ by a series of the following

kind $(a + bx^m + cx^{2m} + \&c.)^{n+1} \times x^d \times (a + \beta x^m + \gamma x^{2m} + \&c. \text{ in infinitum})$. Sir ISAAC NEWTON, by serieses of the same kind, found the fluents of fluxions of this formula $(a + bx^m + cx^{2m} + \&c.)^l \times (e + fx^m + gx^{2m} + \&c.)^m \times \&c. x^{p-1} \dot{x}$; the same principle is extended somewhat more general in the *Meditationes*.

8. Mr. JOHN BERNOULLI found the fluent of any fluxion $\int nx = nx - \frac{x^{n+1}}{n+1} + \frac{x^{n+2}}{2 \cdot 3x^2} - \&c.$ from the principles which Mr. CRAIG published for finding the fluents of fluxions involving fluents.

In the *Meditationes* something is added of the convergency of these series; and also,

9. In them a new method is given of finding approximations. Let some terms in the given quantity be much less or greater than the rest; then reduce the quantity into terms proceeding according to the dimensions of the small quantities, or according to the reciprocals of the great quantities, and it is done. If the fluent of the quantity resulting is required, find it from the common methods, if possible; but if not, reduce the terms not to be found into an infinite series, and then find approximate values to each of the terms, &c.

Ex. 1. Let R the radius, and A the arc of a circle whose sine is S and cosine C, and $A \pm e$ an arc of a circle which does not much differ from the arc A, that is, let e be a very small quantity; then will the sine of the arc $A \pm e$ be $S(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2 \cdot 3 \cdot 4} \times \frac{e^4}{R^4} - \frac{1}{2 \cdot 3 \cdot 4 \cdot 5 \cdot 6} \times \frac{e^6}{R^6} + \&c.) \pm C(\frac{e}{R} - \frac{1}{2 \cdot 3} \times \frac{e^3}{R^3} + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} \times \frac{e^5}{R^5} - \&c.)$; and the cosine of the same arc will be $C(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2 \cdot 3 \cdot 4} \times \frac{e^4}{R^4} - \&c. \text{ in infinitum}) \pm S(\frac{e}{R} -$

$$\frac{1}{2 \cdot 3} \times \frac{x^2}{R^3} + \frac{1}{2 \cdot 3 \cdot 4 \cdot 5} \times \frac{x^4}{R^5} - \&c.)$$

Ex. 2. $\text{Log. } (x \pm e) = \int \frac{x}{x \pm e} = \log. x \pm \frac{e}{x} \pm \frac{e^2}{2x^2} \pm \frac{e^3}{3x^3} - \&c.:$

$$\int \frac{x}{a^2 - (x+e)^2} = \int \frac{x}{a^2 - x^2} + \frac{e}{a^2 - x^2} + \&c.$$

Ex. 3. $\int \frac{x}{a^2 + (x-e)^2} = \int \frac{x}{a^2 + x^2} - \frac{e}{a^2 + x^2} + \&c.$

Ex. 4. $\int \frac{y}{\sqrt{(1-(y \pm e)^2)}} = \int \frac{y}{\sqrt{1-y^2}} \pm \frac{e}{\sqrt{1-y^2}} \pm \&c.$

Ex. 5. $\int \frac{y}{\sqrt{1-y^2} + e} = \int \frac{y}{\sqrt{1-y^2}} - \int \frac{e y}{1-y^2} + \int \frac{e^2 y}{(1-y^2)^2} - \int \frac{e^3 y}{(1-y^2)^3} + \&c.$

Ex. 6. Let the fluxion be $\frac{\sqrt{1-cx^2} \dot{x}}{\sqrt{1-x^2}}$, where c is a very small quantity; then, if P be put for $\sqrt{1-x^2}$, the fluxion becomes $\frac{\dot{x}}{P} - \frac{cx^2 \dot{x}}{2P} - \frac{c^2 x^4 \dot{x}}{8P} - \frac{c^3 x^6 \dot{x}}{16P} - \&c.$ of which the fluents will be found $A = \frac{c}{2} \times \frac{P \cdot A - xP}{2} - \frac{c^2}{8} \times \frac{3B - x^3P}{4} - \frac{c^3}{16} \times \frac{5C - x^5P}{6} - \&c.$ where $A = \int \frac{\dot{x}}{P}$, $B = \frac{P \cdot A - xP}{2}$, $C = \frac{3B - x^3P}{4}$, &c.

This is the swiftest converging series for finding the length of the arc of an ellipse nearly circular, which is yet known; for example, let the absciss to the axis beginning from the center = x , the semi-transverse axis of the ellipse be r , its semi-conjugate $1-d$; then will $c = 2d - d^2$, and let the length of the quadrant of the ellipse be required, in this case $x = 1$, and $P = \sqrt{1-x^2} = 0$; and $A = \frac{3.14159, \&c.}{2} = 1.57079$, &c. whence the length required is $1.57079, \&c. \times (1 - \frac{c}{2 \cdot 2} - \frac{1 \cdot 3c^2}{2^2 \cdot 4} - \frac{1 \cdot 3^2 \cdot 5c^3}{2^2 \cdot 4^2 \cdot 6} - \frac{1 \cdot 3^2 \cdot 5^2 \cdot 7c^4}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8} - \&c.)$.

Ex. 7. Let the fluxion be $\dot{x}(a' + a' + (3a'b + b')x + (3ab' + c')x^2 + (b' + d')x^3) = P\dot{x}$, where $a', b', c', \&c.$ are very small quantities; then will $P\dot{x} = ((a + bx)^{3m} + ma + bx^{3m-1} \times (a' + b'x + c'x^2 + d'x^3) + m \cdot \frac{m-1}{2} \frac{a + bx^{3m-2}}{a + bx^{3m-1}} \times (a' + b'x + c'x^2 + d'x^3)^2 + \&c.) \dot{x}$,

of which the fluent is $\frac{1}{3m+1 \cdot b} \frac{a + bx^{3m+1}}{a + bx^{3m+1}} + m(a + bx)^{3m-1} \times \left(\frac{a'}{(3m+1)b} x^3 + \frac{c' - 3aA}{3mb} x^2 + \frac{b' - 2aB}{(3m-1)b} x + \frac{a' - aC}{(3m-2)b} \right) + \&c.$ where the letters A, B, C, &c. denote the preceding co-efficients.

10. M. EULER and others, reduced the series $Ax^r + Bx^{r+1} + Cx^{r+2} + \&c.$ into a series $A' \sin. r\alpha + B \sin. r + s\alpha + \&c.$ &c. where α denotes the arc of a circle, whose sine is ax , &c. It may be easily reduced into infinite other serieses proceeding according to the dimensions of quantities, which are functions of x ; but it is most commonly preferable to reduce it into serieses proceeding according to the sines, cosines, tangents, or secants of the arcs of circle, which sines, &c. can immediately be procured from the common tables.

It has been observed in the first part, that to find the root of an equation, an approximate value much more near to one root of the equation than to any other must be given. In this part it is further observed, that serieses deduced from expanding given quantities, so as to proceed according to the dimensions of the unknown or variable quantities, will not converge if the unknown quantities be greater than the least roots of the above-mentioned equations; and that they will not converge much, unless the unknown quantities have a small proportion to the least roots: and if the given quantities be expanded into serieses descending according to the dimensions of the unknown quantities, then the serieses resulting will not converge if the greatest

greatest roots of the equations before-mentioned be greater than the unknown quantities; and unless the unknown quantities have a great ratio to the greatest roots the serieses will converge slowly: for example, the serieses $\int \frac{x}{1+x} = x - \frac{1}{2}x^2 + \&c.$, $\int \frac{x}{1+x^2} = x - \frac{1}{2}x^2 + \frac{1}{4}x^4 - \&c.$, $\int \frac{y}{\sqrt{1-y^2}} = y + \frac{1}{2}y^3 + \&c.$ will never converge if x , z , or y , be greater than 1; but will always converge when less than ± 1 or $\pm 1\sqrt{-1}$ the least or only roots of the equations $1+x=0$, $1-y^2=0$, and $1+z^2=0$. The series $y + \frac{1}{2}y^3 + \&c.$ will always converge when y is situated between $+1$ and -1 , in which case alone it is possible. The same is true also of a series arising from expanding the $\int(ax^m + bx^{m-1} + cx^{m-2} + \&c.)^{1/x}$ into a series proceeding according to the dimensions of x , if the equation $ax^m + bx^{m-1} + cx^{m-2} + \&c. = 0$ has only two possible roots α and $-\alpha$, which are less in the manner before-mentioned than any impossible root contained in it.

If in either of the above-mentioned serieses the unknown quantity x , z , or y , has a great proportion to 1, the series will converge very slow; for example, if $x=1$, ten thousand numbers at least are to be calculated, to procure the sum of the series true to four figures; therefore, in these and most other serieses it is necessary first to find a near value, viz. when x either $=z$, when ϵ is very small; or $=e$, when z is very small; and then write $z+\epsilon$ for x in the quantity, and reduce it in the former case into a series proceeding according to the dimensions of ϵ , in the latter case according to the dimensions of z , and there will arise two serieses, of which the fluents properly corrected, viz. by adding the fluent contained between the values z and ϵ to the latter, and that between z and z to the former, will give the same fluent.

fluent. The first term of the series, in which e is supposed very small, will be the fluent of the given fluxion, when $x=z$.

11. If a fluxion $P\dot{x}$, where P is a function of x , be transformed into another $Q\dot{z}$, where Q is a function of z , and they be reduced into serieses A and B , proceeding according to the dimensions of x and z respectively; find α and π , correspondent values of the quantities x and z ; then in ascending serieses, if α has a less ratio to the least root of the equation $P=0$, than π has to the least root of the equation $Q=0$, the series A (*exceptis excipiendis*) will converge swifter than the series B .

12. Dr. BARROW, in some equations, expressing the relation between the absciss x and ordinate y , found y in the two first terms of x , viz. $y=a+bx$, which is an equation to the asymptotes of the curves. Sir ISAAC NEWTON, from an algebraical equation given, expressing the relation between y and x , found a series proceeding according to the dimensions of x , expressing y in terms of x . M. LEIBNITZ performed the same problem by assuming a series $Ax^r+Bx^{r+r}+Cx^{r+2r}+\&c.$ with general co-efficients, and substituting this series for y in the given equation, &c. from equating the correspondent terms he deduced the indexes and co-efficients. M. DE MOIVRE, Mr. MAC LAURIN, &c. observed, that when the highest terms of the given equations have two or more (m) divisors equal; for example, $(y-ax^r)^m$; to which we must add, and when a value of y in this case is required nearly equal to Ax^r , a series $Ax^r+Bx^{r+\frac{r}{m}}+\&c.$ is to be assumed, whose indexes differ only by $\frac{r}{m}$, &c. if otherwise they would differ by r .

This reduction seldom answers any other purpose than finding the fluents of fluxions as $\int y\dot{x}$, &c.; or the asymptotes, &c.

of

of curves, which depend on some of the first terms of the series; but will very seldom be used for finding the roots of an equation; the rule of false, or method given by VIETA, will ever be substituted in its stead.

13. The values of x may be required between which the above-mentioned series $Ax^m + Bx^{m+r} + Cx^{m+2r} + \&c.$ will converge, as the infinite series answers no purpose when it diverges. First, if an ascending be required, write for y in the given algebraical equation an infinite quantity, and find the roots of x in the equation thence resulting $P=0$; which for y write in the same equation, and find the roots of x in the resulting equation which contain irrational quantities, viz. if one root be $x=a$; then let it contain $(x-a)^m$, where m is not a whole number; find the roots of the equations resulting from making every irrational function of (x) contained in the given equation $=0$, there being no irrational function of y contained in it; then, if x be greater than the least root $=0$ of the above-mentioned equations, the series will not converge; but if it be a series descending according to the dimensions of x , and x be less than the greatest root of the above-mentioned equations, the series will not converge.

In interpolating serieses to investigate the fluent contained between two values a and b of the fluxion $(Ax^m + Bx^{m+r} + \&c.) / x$, it is preferable to make the abscissæ begin from every one of the above-mentioned roots contained between a and b .

Most commonly these serieses will not converge unless x be less, &c. than other quantities not investigated by this rule.

14. Sir ISAAC NEWTON gave an elegant example of this rule in the reversion of the series, $y = ax + bx^2 + cx^3 + \&c.$ from which the investigation of the law of the series has never been attempted. In the year 1757 I sent the first edition of my

Meditationes Algebrae to the Royal Society, and published it in 1760, and afterwards in 1762, with another part added, on the Properties of curves, under the title of *Miscellanea Analytica*, in all which was given the law of a series for finding the sum of the powers of the roots of an equation from its co-efficients. That great mathematician M. LE GRANGE and myself printed about the same time an observation known to me at the time that I printed the above-mentioned book, that the law of its powers and roots, if it proceeds in *infinitum*, is the same; from which series of mine, with great sagacity, M. LE GRANGE found the law which Sir ISAAC NEWTON's reversion of series observes. In the *Meditationes* the law is given, and the series is made to proceed according to the dimensions of x , &c.

15. It is asserted in the *Meditationes*, that in most equations of high dimensions, unless purposely constituted, the sum of the terms which, from the given hypothesis, become the greatest, being supposed $= 0$, only an approximate to the value Ax^r of y in the resulting equation can by the common algebra be deduced. In this case the approximate to the quantity A is to be found so near as the approximate value of the quantity sought requires; or perhaps it is preferable to correct in every operation the approximate values of the quantities A, B, C , &c. in the series required $A'x^r + B'x^{r+r} + C'x^{r+r} + \&c.$

In the equation the quantity $x \pm e$ may be substituted for x , and from the equation resulting a series expressing the value of y may be found, proceeding either according to the dimensions of the quantity z , or its reciprocals, according to the conditions of the problem.

16. If there are more than one (n) equations having $(n+1)$ unknown quantities (x, y, z , &c.) in each of the equations for y ,

x , &c. write respectively Ax^n , $A'x^n$, &c.; and suppose the terms of each of the equations, which result the greatest from the given or assumed hypothesis $= 0$, and from the resulting equations may be found the first approximates Ax^n , $A'x^n$, &c. either accurately or nearly; then, in the given equations for y , z , &c. write $y' + (A + a)x^n + Bx^{n+1} + \&c. z' + (A' + a')x^n + B'x^{n+1}$, where a , a' , &c. are very small quantities; and suppose the terms of each of the equations which become greatest from the above-mentioned hypothesis respectively $= 0$, and from the equations resulting deduce the quantities a , a' , &c.; n' , m' , &c.; B , B' , &c.; and so on: or assume $y = (A + 1a + a1 + \&c.)x^n + (B + 1b + b1 + \&c.)x^{n+1} + \&c.$; $z = (A' + 1a' + a'1 + \&c.)x^n + (B' + 1b' + b'1 + \&c.)x^{n+1} + \&c.$; substitute these quantities for their values in the given equations, and from equating the correspondent terms of the resulting equations may be deduced the quantities required.

The differences of the indexes n' , &c. m' , &c. may be deduced by writing x^n , x^m , &c. for y , z , &c. in the given equations, from the differences of the indexes of the quantities resulting. The same principles may be applied in finding the above-mentioned differences, when two or more values are Ax^n , &c. which were applied in a like case to one equation having two unknown quantities.

The same principles which discover the cases in which a series deduced from an equation having two unknown quantities will converge, may be applied for the same purpose to these series.

17. In the equations for x , y , z , &c. write respectively $x' + e$, $y' + f$, $z' + g$, &c.; and from the equations resulting find y' , z' , &c. in serieses either proceeding according to the dimensions

Q^2 of

of the quantities $z, f, g, \&c.$; or according to the dimensions of the quantity x^r , as the conditions of the problem require.

18. Given one or more (n) algebraical equations involving $(n+m)$ unknown quantities, one unknown quantity (y) may be expressed by a series proceeding according to the dimensions of the $m-1$ remaining ones $(x, z, v, \&c.)$, in which any dimensions of $z, v, \&c.$ assumed may be supposed to correspond to (l) dimensions of the quantity (x) .

19. In a fluxional equation of the m^{th} order, expressing the relation between x, y , and their fluxions, where $\dot{x}$ is constant, Mr. EULER substitutes in the given equation for $\dot{y}^m, \dot{y}^{m-1}, \dot{y}^{m-2}, \dot{y}^{m-3}, \&c.$ respectively $Ax^{n-m}\dot{x}^m, \frac{A}{n-m+1}x^{n-m+1}\dot{x}^{m-1} + a\dot{x}^{m-1},$

$$\frac{A}{(n-m+1)(n-m+2)}x^{n-m+2}\dot{x}^{m-2} + ax\dot{x}^{m-2} + b\dot{x}^{m-2}, \frac{A}{(n-m+1)(n-m+2)}x^{n-m+3}\dot{x}^{m-3} + \frac{1}{2}ax^2\dot{x}^{m-3} + bx\dot{x}^{m-3} + c\dot{x}^{m-3}, \&c. \text{ where}$$

$a, b, c, \&c.$ are any quantities to be assumed in such a manner as the conditions of the problem require; from supposing the aggregate of the terms of the resulting equation, which are the greatest, $=0$, may be deduced the first approximate Ax^r , or else (as is beforementioned) $A'x^r$ a near approximate to Ax^r , and by proceeding as in algebraical equations another approximate may be found, and so on. The same may be found by assuming $y = Ax^r + Bx^{r+r} + Cx^{r+2r} + \&c. + ax^r + bx^{r-1} + cx^{r-2} + \&c.$ or $y = (A + 1a + a1 + \&c.)x^r + (B + 1b + b1 + \&c.)x^{r+r} + (C + 1c + c1 + \&c.)x^{r+2r} + \&c. + ax^r + bx^{r-1} + cx^{r-2} + \&c.$ and substituting it and its fluxions for their values $y, \dot{y}, \ddot{y}, \&c.$ in the given equation, and supposing the aggregate of each correspondent terms, which do not very much differ from each other, $=0$; from the resulting equations can be deduced the co-efficients $A, B, C, \&c.$; or $A, 1a, a1, \&c.$; $B, 1b, b1, \&c.$; $C, 1c, c1, \&c. \&c.$

In the given equation for y , x , and their fluxions substitute $y' + f$, $x' + g$, and their fluxions, where the quantities f and g , &c. are assumed, so as to render the quantities y' and x' , &c. very small.

In finding the series which expresses the value of y in terms of x , there will always occur as many invariable quantities to be assumed at will as is the order of the fluxional equation, provided the series begins from its first terms; and to find them there will result equations easily reducible to homogeneous fluxional equations, of which the orders do not exceed m .



V. *Experiments on Hepatic Air.*By Richard Kirwan, *Esq.* F. R. S.

Read December 22, 1785.

HEPATIC Air is that species of permanently elastic fluid which is obtained from combinations of sulphur with various substances, as alkalis, earths, metals, &c. It possesses many peculiar and distinct properties; among which the most obvious are, a disagreeable characteristic smell emitted by no other known substance; inflammability, when mixed with a certain proportion of respirable or nitrous air; miscibility with water, to a certain degree; and a power of discolouring metals, particularly silver and mercury. These properties were first discovered by that incomparable analyst M. SCHEELÉ.

This air acts an important part in the œconomy of nature. It is frequently found in coal-pits; and the truly excellent and ever to be regretted M. BERGMAN has shewn it to be the principle on which the sulphureous properties of many mineral waters depend, and thus happily terminated the numerous disputes which the obscurity of that subject had occasioned. There is also great reason to think, that it is the peculiar product of the putrefaction of many, if not all, animal substances. Rotten eggs and corrupt water are known to emit the smell peculiar to this species of air, and also to discolour metallic substances

stances in the same manner. M. VIELLARD has lately discovered several other indications of this air in putrefied blood.

Yet, deserving as this substance appears to be of a thorough investigation, it has as yet been very little attended to. The experiments of M. BERGMAN have not been sufficiently numerous, and thereby led him into some mistakes. Dr. PRIESTLEY has intirely overlooked it. The researches of the ingenious M. SENNEBIER, of Geneva, have indeed been very extensive; but as, for particular reasons, he operated on this air over water (by which it is in great measure absorbed) instead of quicksilver, his conclusions appear in many respects objectionable, as will be seen in the sequel. The experiments I have now the honour of laying before the Society were all made over quicksilver, and several times repeated.

SECTION I.

Of the Substances that yield Hepatic Air, and the means of obtaining it.

It is well known, that *saline* liver of sulphur is formed, in the dry way, of a mixture of equal parts of vegetable or mineral alkali and flowers of sulphur, melted together by a moderate heat, in a covered crucible. I examined the circumstances of its formation, and observed, that when this mixture was slightly heated, it emitted a bluish smoke, which gradually grew whiter as the heat was increased, and at last, when the mixture was perfectly melted, and the bottom of the crucible slightly red, became perfectly white and inflammable. To examine the nature of this smoke, I made a pretty pure fixed alkali, by deflagrating equal parts of cream of tartar and nitre

nitre in a red-hot crucible in the usual way; and mixing this salt perfectly dry with flowers of sulphur in much smaller quantity, as I believe (for I did not weigh the salt, least it should, during the weighing, attract moisture) I gradually heated the mixture in a small coated glass retort, and received the air proceeding from it over quicksilver.

The first portion of air that passed with a very gentle heat was that of the retort itself, slightly phlogisticated. It amounted to 1,5 cubic inches, and with Dr. PRIESTLEY's nitrous test (that is, an equal measure of nitrous air) its goodness was 1,29. It contained no fixed air.

The second portion of air obtained by increasing the heat amounted to about 18 cubic inches. It was of a reddish colour, and seemed a mixture of nitrous and common air. It slightly acted on the mercury.

The third portion consisted of 20 cubic inches, and appeared to be of the same kind as the last, but mixed with a little fixed air.

This was succeeded by 64 cubic inches of almost perfectly pure fixed air; and the bottom of the retort being now red, some sulphur sublimed in its neck. When all was cold, liver of sulphur was found in the bulb of the retort.

Hence we see, that the blue smoke consists chiefly of fixed air, and the white or yellow smoke of sulphur sublimed; and that no hepatic air is thus formed; nor vitriolic air, unless the retort be so large as to contain a sufficiency of common air to admit the combustion of part of the sulphur.

2dly, That the aerial or any other acid, combined with the alkali, must be expelled before the alkali will combine with the sulphur. Liver of sulphur exercises a strong solving power on the earth of crucibles, and readily pierces through them.

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The above experiment seems to shew that liver of sulphur will not yield hepatic air without the addition of an acid; and I believe this to be true when the experiment is made in the dry way, and nearly so in the moist way; for having added 200 grains of sulphur to a concentrated solution of strong caustic vegetable alkali, by a strong and long-continued heat I obtained only 1 cubic inch of hepatic air; yet it is well known, that a strong solution of liver of sulphur constantly emits an hepatic smell, even in the temperature of the atmosphere; and the substance so emitted contains so much hepatic air as to discolour silver and lead, and even their solutions; which shews, that an incomparably small quantity of this air is capable of producing this effect. To discover whether this extrication of hepatic air might be caused by the deposition of fixed air from the atmosphere, I threw some pulverised calcareous hepar into aerated water, and by the application of heat endeavoured to obtain hepatic air, but in vain: and, indeed, the very circumstance that the hepatic smell, and its effects, are always strongest the first instant that a bottle of the hepatic solution is opened, seems to indicate that fixed air is no way concerned in its production.

The best liver of sulphur is made of equal parts of salt of tartar and sulphur; but as about one-fifth of the salt of tartar consists of air which escapes during the operation, it seems, that the proportion of sulphur predominates in the resulting compound; yet as some of the sulphur also sublimes and burns, it is not easy to fix the exact proportion. 100 grains of the best, that is, the reddest liver of sulphur, afford, with dilute marine acid, about 40 cubic inches of hepatic air, in the temperature of 60°: a quantity equivalent to about 13 grains of sulphur, as will be seen in the sequel.

The *marine* acid is the best adapted to the production of hepatic air. If the concentrated *nitrous* acid be used, it will afford nitrous air; but having diluted some nitrous acid, whose specific gravity was 1,347, with 20 times its bulk of water, I obtained, with the assistance of heat, as pure hepatic air as with any other acid.

The concentrated *vitriolic* acid, poured on liver of sulphur, affords but little hepatic air without the assistance of heat, though it instantly decomposes the liver of sulphur; and it is partly for this reason that the proportion of air is so small; for it is during the *gradual* decomposition of sulphureous compounds that hepatic air is produced.

Distilled *vinegar* extricates this air in the temperature of the atmosphere; but it is not pure, its peculiar smell being mixed with that of vinegar.

The *acid of sugar* also produced some quantity of this air in the temperature of 59°.

20 grains of *sedative salt*, or acid (as it should more properly be called) dissolved in an ounce of water, being poured on liver of sulphur, afforded hepatic air only when in a boiling heat, or nearly so.

Neither the *aërial* nor *arsenical* acids produce any.

Liver of sulphur is soluble not only in water but in spirit of wine, and in caustic volatile alkalies; and the colour of both solutions is red. Sulphur is precipitable from the former by the addition of water or of an acid, but from the latter only by an acid.

Having made some liver of sulphur, in which the proportion of sulphur much exceeded that of the alkali, I poured on part of it some oil of vitriol, whose specific gravity was 1,863: by this means I obtained hepatic air, so loaded with

with sulphur, that it deposited some in the tube through which it was transmitted, and on the upper part of the glass receiver. This air I transferred to another receiver; but though it was then perfectly clear and transparent, and amounted to 6 cubic inches, yet the next morning the inside of the glass was thickly lined with sulphur, and the air reduced to 1 cubic inch, which was pure vitriolic air. Hence it appears, first, that a species of elastic fluid may exist in a state intermediate between the ærial and the vaporous, which is not permanently elastic like air, nor immediately condensed by cold like vapour, but which, by the gradual loss of its specific heat, may be reduced to a concrete form. 2dly, That so large a quantity of sulphur may be combined with vitriolic air, as to enable it to exhibit the properties of hepatic air, for some time at least. A mixture of three parts pulverised quick-lime and one part sulphur, heated to whiteness in a covered crucible for one hour, became of a stony hardness, and being treated with marine acid, afforded hepatic air. If a piece of this stone be heated in pure water it becomes bluish, and hence the origin of the blue marles generally found near hot sulphurated waters.

A calcareous hepar may also be formed in the moist way, as is well known.

Magnesia free from fixed air, heated in the same manner with sulphur, afforded no hepatic air when an acid was poured on it.

I also procured this air from a mixture of three parts *filings of iron* and one of sulphur, melted together, and treated with marine acid. It is remarkable, that this sulphurated iron, dissolved in marine acid, affords scarce any inflammable, but mostly hepatic air.

A mixture of equal parts filings of iron and sulphur, made into a paste with water, after heating and becoming black, afforded hepatic air when an acid was poured on it; but this hepatic air was mixed with inflammable air, which probably proceeded from the uncombined iron. After a few days, this paste lost its power of producing hepatic air.

M. BERGMAN has remarked, that combinations of sulphur with some other metals yield hepatic air.

I attempted extracting air from a mixture of oil of olives with caustic vegetable alkali. It immediately whitened, and on applying heat effervesced so violently as to pass over into the receiver: nor had I better success on adding an acid, as I might well foresee. The event was different when on a few grains of sulphur I poured some of the oil, and heated them in a phial with a bent tube; for as soon as the sulphur melted, the oil began to act on it, grew red, and emitted hepatic air, similar to that produced by other processes.

I also obtained this air in great plenty from a mixture of equal parts sulphur and *pulverised charcoal*, out of which its adventitious air had been as much as possible expelled by keeping it a long time heated to redness, in a crucible on which a cover was luted, with a small perforation to permit the air to escape. This air was inflammable, as appeared by holding a lighted candle before it during its emission; yet it is hardly possible to free charcoal wholly from foreign air, for it soon re-attracts it when exposed to the atmosphere.

This last mixture, when distilled, affording a large quantity of hepatic and some inflammable air, without the addition of any acid, I imagined, that as the retort was only half full, it might contain a sufficiency of atmospheric air to admit the combustion of part of the sulphur, and thus furnish the neces-

sary acid; but when I filled the retort with air phlogisticated by the nitrous test unto 1,8, and in this air distilled the above mixture, the result was exactly the same as when the retort was filled with atmospheric air.

Six grains of *pyrophorus*, made of alum and sugar, effervesced with marine acid, and afforded 2,5 cubic inches of hepatic air. This pyrophorus had been made six years before, and was kept in a tube hermetically sealed, and for many summers exposed to the strongest light of the sun. It was so combustible, that some grains of it took fire while it was introduced into the phial out of which the hepatic air was expelled.

A mixture of two parts *white sugar* (previously melted in order to free it from water) with one part sulphur, when heated to about 600 or 700 degrees, gave out hepatic air very rapidly. This air had a smell much resembling that of onions; it contained no fixed air, nor saccharine, or other acid. But sugar and sulphur, melted together, gave out no hepatic air when treated with acids. Water, spirit of wine, and marine acid, decompose this mixture, dissolving the sugar, and leaving the sulphur.

A mixture of sulphur and *plumbago* afforded me no hepatic air.

I then tried whether sulphur could combine with elastic fluids, and the results were as follows.

12 grains of sulphur, heated in a retort, filled with metallic *inflammable* air, afforded no hepatic air; though when the retort was cold, and for some time exposed to the air, it smelled of hepatic air. It is true, the heat I applied might be insufficient; for the inflammable air passing over with a slight heat, the mercury ascended so high into the neck of the receiver, that, fearing the rupture of the retort, I was obliged to interrupt the
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the operation. I had no better success when the sulphur, previous to its distillation, had been moistened with marine acid.

Again, I exposed 18 grains of liver of sulphur to six cubic inches of *fixed air*, thermometer 70°, for four days. The liver of sulphur was somewhat whitened on the surface; the air had not an hepatic smell, but rather that of bread. It was not converted into phlogisticated air, but seemed to have taken up some sulphur, which lime-water separated. It was not in the least diminished, and therefore seems to have received an addition of hepatic air, or rather of sulphur.

I also exposed a quantity of *sulphureo-martial paste* to fixed air, for five days. The fixed air was not diminished, but received a slight accession of inflammable air. The paste itself, taken out of this air, and exposed to the atmosphere, heated strongly.

Lastly, I exposed 3 grains of sulphur to about 12 cubic inches of *marine air*. It was not diminished in four days; nor was the sulphur sensibly. On adding one cubic inch of water to this air, it was absorbed all to one inch, and this had an hepatic smell; so that neither was the sulphur decomposed, nor the marine acid converted into inflammable air. The water had also an hepatic smell, and evidently contained sulphur; for it precipitated the solution of silver *brown* mixed with white, and the nitrous solution of copper *reddish brown*, and when vegetable fixed alkali was dropped into it, let fall a white precipitate, namely, the sulphur.

SECTION II.

Of the general Characters of Hepatic Air.

I found the absolute weight of this air by weighing it in a glass bottle, previously exhausted by Mr. HURTER's new improved air-pump, whose effect is so considerable as to leave in general only $\frac{1}{1000}$ and frequently but $\frac{1}{10000}$ part of unexhausted air. This bottle contained 116 cubic inches nearly; and this quantity of hepatic air weighed 38,58 grains, the thermometer being then $67^{\circ},5$, the barometer 29,94, and M. SAUSSURE's hygrometer 84° , the weight of 116 cubic inches of atmospheric air being at the same time 34,87 grains; hence a cubic foot of hepatic air weighs, in these circumstances, 574,7089 grains, and 100 cubic inches of it weigh about 33 grains; and its weight, relatively to that of common air, is as 10000 to 9038*. This hepatic air was extracted from artificial pyrites by marine acid.

The inflammability of this air has been already mentioned. It never detonates with common air; nor can it be fired, in a narrow-mouthed vessel, unless mixed with a considerable proportion of this air. M. SCHEELE found it to inflame when mixed with two-thirds of this air. According to M. SENNEBIER.

* Hence the weight which I have assigned to common air in my first paper, after M. FONTANA, is evidently erroneous; and, indeed, by that determination common air would not be even 700 times lighter than water, in the temperature of 55° , and the barometer 29,5, which contradicts all barometrical and aerostatic experiments: and I cannot omit mentioning the very near agreement of the weight of common air here found with that resulting from the calculation of Sir GEORGE SHUCKBURN, it is so great as to differ only by 2 grains in a cubic foot.

it cannot be fired by the electric spark, even when mixed with any proportion of respirable air. I found a mixture of one part of hepatic air and 1,5 of common air to burn blue, without flashing or detonating. During the combustion sulphur is constantly deposited, and a smell of vitriolic air is perceived. A mixture of half hepatic and half nitrous air burns with a bluish, green, and yellow lambent flame; sulphur is also deposited, and in proportion as this is formed, a candle dipped in this air burns more weakly, and is at last extinguished. A mixture of two parts nitrous and one of hepatic air partially burns, with a green flame, and a candle is extinguished in the residuum, which then reddens on coming in contact with atmospheric air. I made a mixture of one part nitrous and one part hepatic air, and to this admitted one part also of common air; the instant the common air was introduced, sulphur was precipitated, and the three measures occupied the space of 2,4 measures; this burned on the surface with a wide greenish flame, but the candle was extinguished when sunk deeper.

A mixture of four parts common air and one part hepatic burned blue and rapidly; but a mixture of one part dephlogisticated and one part hepatic, which had stood eight days, went off with a report as loud as that of a pistol, and so instantaneously that the colour of the flame could scarce be discerned.

Every species of hepatic air turns the *tincture of litmus* red. — M. BERGMAN seems to think, that, if this air were washed, it would not produce this effect; yet, when I had passed two measures of it through one of water, or when I had boiled it out of water impregnated with it, or even when I passed that which had already reddened litmus, into a fresh quantity of litmus, it still preserved the same property, which I therefore consider as essential to it.

With

With respect to *solubility in water*, hepatic air extracted from different materials differ considerably. In the temperature of 66° , water dissolves, by slight agitation, two-thirds of its bulk of alkaline or calcareous hepatic air, extracted by marine acid; three-fourths of its bulk of martial hepatic air, extracted by the same acid; eight-tenths of that extracted by means of the concentrated vitriolic acid, or the dilute nitrous or saccharine acids in the temperature of 60° ; seven-tenths of sedative hepatic air; nine-tenths of acetous hepatic air, and of that afforded by oil of olives; and its own bulk of that produced from a mixture of sugar and sulphur. In general, I imagined that which required most heat for its production to be most soluble: though in some instances, particularly that of acetous hepatic air, that circumstance does not take place.

But the most remarkable phenomenon attending the union of hepatic air with water is, that it is not permanent. Even water, out of which its own air had been boiled, in a few days after saturation with hepatic air grows turbid, and in a few weeks deposits most of it in the form of sulphur, though the bottle be ever so well stopped, or stand inverted in water or mercury. Yet water no way decomposes hepatic air by absorbing it; for the part left unabsorbed by a quantity of water is absorbable by a larger quantity of water, and burns like other hepatic air. Heat does not expel this air from water, until carried to the degree of ebullition.

No species of hepatic air, which I have examined, precipitates lime from *lime-water*, except the carbonaceous; and even this scarcely produces a sensible precipitation, unless a large quantity of it be made to pass through a small quantity of lime-water.

The solution of *acetous baro-selenite*, (that is, ponderous earth dissolved in distilled vinegar) is rendered brown and turbid by this air, but that of *marine baro-selenite* is not altered; nor are the solutions of other earths. Metallic solutions are affected by it in the same manner as by hepatic water, of which I shall treat in the fifth section.

But of all the tests of hepatic air, the most delicate and sensible is the *solution of silver in the nitrous acid*. This, according as the nitrous acid is more or less saturated with silver, becomes black, brown, or reddish brown, by contact with hepatic air however mixed with any other air or substance. When the acid is not saturated, or is in large proportion, the brown or black precipitate, which is nothing but sulphurated silver, is re-dissolved.

It should also be remarked, that all hepatic air is somewhat diminished by long standing on mercury, whose surface is then blackened by it. This is particularly the case of carbonaceous hepatic air, which certainly carries over and volatilizes part of the charcoal from which it is extracted, especially that portion of air which comes over in the greatest heat; this it deposits on the addition of water.

SECTION III.

Of the Action of Hepatic and other Aerial Fluids on each other.

Six cubic inches of common and six of hepatic air being mixed with each other, and standing over mercury for eight days, were not in the least altered in their dimensions or otherwise; though a diminution of a $\frac{1}{150}$ th part might be perceived. The mercury was slightly blackened. The event was the same when

when three measures of common and one of hepatic air were used. Water took up the hepatic air. No fixed air was found.

Five measures of hepatic, and five of *dephlogisticated* air so pure that one measure of it and two of nitrous air made only three-tenths of a measure, remained unaltered for eight days, the mercury only being blackened. No fixed air was produced, nor the dephlogisticated air phlogisticated. When the mixture was fired, it went off all at once with a loud report.

Four measures of phlogisticated and four of hepatic air remained undiminished for sixteen days: water then took up the hepatic, and left the phlogisticated air.

Four measures of *inflammable* and four of hepatic air remained unaltered for six days.

Two measures of hepatic and two of *marine acid* air suffered no diminution in three days. The mercury on which they stood was not blackened. Water took up both, and precipitated the solution of silver black.

The same quantity of hepatic and *fixed* air remained four days without any sensible diminution. Four measures of water absorbed the greater part of both, had an hepatic smell, precipitated lime from its solution, and also silver, as usual. The residuum extinguished a candle.

But *vitriolic*, *nitrous*, and *alkaline* airs had very remarkable effects on hepatic air.

Two measures of hepatic being introduced to two of *vitriolic* air, a whitish yellow deposition immediately covered both the top and sides of the jar, and both airs were, without any agitation, reduced to little more than one measure; but the opacity of the incrustated glass prevented my then ascertaining the diminution with precision. Hence I repeated this experiment more at large, in the following manner. To five measures of vitriolic air (each measure containing a cubic inch) I added one

of hepatic air. In less than a minute, without any agitation, the sides of the glass were covered with a whitish scum, which seemed moist, and a diminution took place of more than one measure. In four hours after, I introduced a second measure of hepatic air, which was followed by a similar diminution and deposit. The next day I added three more measures of this last, at the interval of four hours between each; and still finding a considerable diminution after each, I the following day added another measure; the diminution produced by this last appeared to me not to exceed one measure. I then poured off the residuary air into another jar, and found it not to exceed three measures; so that here eleven measures, namely, five of vitriolic and six of hepatic air, were reduced to three. Into one measure of this residuary air I introduced a lighted candle: it was immediately quenched. To the two remaining measures I added one measure of water: by agitation it took up four-tenths of its bulk. To part of the remainder I added nitrous air, which had no effect upon it. Another part of it extinguished a candle. It had not a vitriolic smell.

The water which had taken up four-tenths of its bulk of this air did not precipitate lime; nor did it affect acetous baro-felenite in less than a quarter of an hour, and then produced a very slight cloud. It sensibly reddened litmus, and precipitated the solution of silver white; and hence it appears to have taken up a very minute portion of vitriolic acid. And what was not taken up by water seems to have been mere phlogisticated air.

I afterwards washed the sulphur, which coated the jar, with distilled water. This water slightly reddened litmus, precipitated not only the acetous, but also marine baro-felenite copiously, as well as marine and nitrous selenite; also the nitrous solutions of silver, lead, and mercury, all white. It even

precipitated lime from lime-water, forming a cloud in it, which neither the fixed nor volatile acid of vitriol can produce. Hence this water contained nothing hepatic; but, on the contrary, a considerable proportion of the aërial and vitriolic acids*.

With *nitrous air* I made the following experiments. First, I found that two measures or cubic inches of nitrous and two of hepatic air were little altered when first mixed, even by agitation; but after thirty-six hours both were reduced to nearly one-third of the whole, but something more. Yellow particles of sulphur were deposited both on the mercury, and on the sides of the jar, but the mercury was not blackened. The residuary air had still an hepatic smell, and was somewhat further diminished by water; and in the unabSORbed part a candle burned naturally. The water had all the properties of hepatic water.

Perceiving by this experiment that I had not employed enough of nitrous air to condense the hepatic perfectly, to eight cubic inches of hepatic air I added nine of nitrous air, all at once; a yellowish cloud instantly appeared, a slight white scum was deposited on the sides of the jar, and the whole seemed diminished about two cubic inches, or between one-ninth and one-eighth, the temperature of the room being then 72° . I then laid by the mixture, and in forty-eight hours after, I found the whole reduced to six cubic inches, and the top and sides of the jar covered with a white cake of sulphur, the heat of the room being constantly kept between 60° and 70° . Finding the diminution to reach no further in

* Note, the vitriolic acid air here employed was the purest possible; it was extracted from sulphur distilled with precipitate *per se*.

twenty-four hours more, I examined the residuary air. It exhibited the following appearances.

1°, It had the smell of alkaline air pretty strongly; at least that smell issued from the jar that contained it after the air itself was poured into another jar.

2°, A candle burned in it naturally.

3°, It did not affect tincture of litmus or lime-water, or acetic barytes.

4°, No species of air had any effect on it except the dephlogisticated, with which it produced a slight redness and diminution.

5°, It produced a slight white precipitate in solution of silver.

It is plain, this air is the same as that which Dr. PRIESTLEY calls *dephlogisticated nitrous air*, and which, I think, may more properly be called *deacidified nitrous air*. A further examination of it would lead me too far from the present subject: I shall therefore defer it until another opportunity.

As it appeared to me, from the experiment mentioned in the second section (in which I found sulphur precipitated from a mixture of nitrous and hepatic air, immediately after the admission of common air) that an uncombined acid in the nitrous air was the cause of the precipitation of sulphur; I attempted depriving nitrous air of any loose acid it might contain, before I should mix it with hepatic air.

1st, I made some nitrous air from silver very carefully over boiled and filtered water, and found it to contain an acid, for it strongly reddened tincture of litmus.

2dly, I admitted alkaline air to this nitrous air until it no longer caused any cloud, and then washed out the ammoniacal compound in distilled water; after which I transferred this

purified nitrous air to the mercurial tub. It appeared to lose, by privation of its acid, about one-sixth of its bulk; and it was diminished by common air just in the same manner as unpurified nitrous air is.

Then to 8 cubic inches of this purified nitrous air I admitted all at once 7 cubic inches of hepatic air. No cloud, diminution, or deposit, appeared; but in six hours after (the temperature of the room being all the time at 76°), the whole was reduced to 5 cubic inches; the diminution went no further eighteen hours after. Sulphur, much whiter than in the former experiments, was deposited, and both in this and in the former experiments that part of it which, by the rising of the mercury, was intercepted between it and the jar, was of a yellow and red shining colour, and not black as that deposited on mercury usually is. The residuary air flashed with so much vehemence as to extinguish the candle dipped into it, by the violence of the blast. The flame was exceeding white and vivid; but it did not detonate in the least, but rather resembled dephlogisticated air. The jar out of which it had been transferred had a sharp alkaline smell.

This air was not in the least diminished by nitrous air, even when heated to 150 degrees; which heat I contrived to produce by passing the upper part of the jar that contained this air into another wider jar, furnished with a perforated cork bottom, and filling this with water heated to that degree.

Water poured into the jar in which the sulphur was deposited, produced a bluish white cloud in solution of silver, though insipid to the taste.

Hence it appears to me, that, whatever this air may be, it had been deacidified by hepatic air still more perfectly than that

that in which a candle burns naturally; and that it is by no means dephlogisticated.

Lastly, *Alkaline* and hepatic airs, perfectly pure and mixed in proper proportions, would probably destroy each other completely, though I have not been able to effect this intirely. Six measures of hepatic air from liver of sulphur and 6 of alkaline air immediately throw up a white cloud, leave a whitish scum on the sides of the jar, and are reduced to about 1 measure. On adding water this is reduced to about one-half; and in this I found a candle to burn naturally; but the following experiments, being made with more care, prove that this residuary air was only the common air of the vessels.

To 6 cubic inches of calcareous hepatic air I admitted, all at once, 7 of alkaline air; a white cloud and a little white scum at first appeared; but in a few seconds the whole was reduced to six-sevenths of a cubic inch; and on adding 2 measures of water, only one-ninth of a cubic inch of air remained. This could not be inflamed. The water, thus impregnated, precipitated a solution of silver black. In this experiment great care was taken to have each of the mixed airs as pure as possible, and the alkaline was admitted all at once, instead of by different portions, merely with that view; and it is probable, that, if the due proportion were hit upon, nothing would remain. The scum appears to be almost liquid, and as soon as the jar is emptied of mercury, it breaks out into a white smoke, with an exceeding sharp urinous smell.

Five measures of martial hepatic air were, upon the admission of $5\frac{1}{4}$ of alkaline air, reduced to something more than one measure, and upon the addition of water there remained but half a measure; and this was inflammable, with detonation; the
inflammable

inflammable air undoubtedly proceeding from the solution of the iron.

Five cubic inches of saccharine hepatic air, mixed with 5 of alkaline air, were diminished more slowly; for after five minutes there still remained 4,5 cubic inches. I then added another measure of alkaline air: in three hours after there remained but 1,25 cubic inches. In passing this residuum through water it was reduced to about half a cubic inch; and this burned with a blue lambent flame, without leaving a vitriolic smell or any deposit on the glass; so that it clearly was inflammable air from the sugar.

I once imagined I had obtained inflammable air from a mixture of alkaline air with hepatic air drawn from liver of sulphur; but I afterwards found this inflammable air proceeded from a very slight contamination of zinc in the mercury over which my airs had been produced; the alkaline air acted on this zinc, and must have produced the inflammable air; for when I afterwards received and mixed these airs over mercury, perfectly purified, I obtained no more inflammable air.

SECTION IV.

Of the Action of Hepatic Air, and Acid, Alkaline, and Inflammable Liquids, on each other.

One measure of oil of vitriol, whose specific gravity was 1,863, absorbed two measures of hepatic air all to one-tenth. The acid was whitened by a copious deposition of sulphur. I also introduced, over mercury, a measure of red *nitrous acid*, whose specific gravity was 1,430, to an equal measure of hepatic air; red

vapours instantly arose, and only one-tenth or one-twelfth of a measure remained in an aërial form; but as the acid acted on the mercury, I was obliged to carry the jar into the water tub, by which means the whole was absorbed: no sulphur was here precipitated.

I repeated this experiment in another manner. Having produced 4,5 measures of hepatic air over mercury, I transferred them to the water tub, and instantly by means of a syphon blew into them one measure of the above concentrated nitrous acid; but though I managed as quickly as possible, the hepatic air was something diminished by contact with the water, before the acid had entered the tube that contained the air. I then stopped the tube with a ground glass stopper, and laid it by for twelve hours; after which interval I found the liquor in the tube white and turbid, and but weakly acid, much water having entered in spite of my endeavours to exclude it. The remaining air slightly detonated on presenting to it a lighted candle, and had an hepatic smell. But as this hepatic air was obtained from sulphureo-martial paste, it does not prove that inflammable air enters into the composition of other hepatic airs, derived from the union of sulphur with substances that do not yield inflammable air.

Finding it so difficult to subject hepatic air to the direct action of the concentrated nitrous acid, I diluted it to that precise degree at which it could not act on mercury without the assistance of heat, and then passed through it an equal bulk of the same hepatic air; the acid was whitened, and eight-tenths of the air absorbed, and the residuum detonated. Repeating the same experiment with hepatic air from liver of sulphur, I found still more of it absorbed by the acid; but the residuum

no longer detonated, but burned with a blue and greenish flame, and sulphur was deposited on the sides of the jar.

Observing this dilute acid to absorb nearly three times its bulk of alkaline hepatic air, I expelled this air from it by heat, but obtained only one-sixth of the air that had been absorbed; and in this a candle burned naturally.

Two measures of alkaline hepatic air, being exposed to one of strong *marine acid*, were absorbed, by slight agitation, all to one-fifth of a measure. A third measure of air being then added, there remained, after some agitation, but half a measure. Sulphur was precipitated as usual; but the mercury over which the acid stood attracted it from the acid; for it was blackened, which did not happen when the former acids were used. The residuum burned just as pure hepatic air.

Distilled vinegar absorbs nearly its own bulk of air, and becomes slightly whitened; but by agitation it may be made to take up about twice its bulk, and then becomes very turbid.

One measure of *caustic vegetable alkali*, whose specific gravity was 1,043, absorbed nearly four measures of alkaline hepatic air. It was at first rendered brown by it; but after some time it grew clear, sulphur was deposited, and the surface of the mercury blackened. This shews that alkalies are not dephlogisticated by silver or other metals, as Mr. BAUME imagined, but only cleared of part of the sulphur, which they commonly contain, it being formed by the tartar vitriolate contained in the plant, and coal, during combustion.

One measure of *caustic volatile alkali*, whose specific gravity was 0,9387, absorbed 18 of hepatic air. If the caustic liquor contained more alkali, it would absorb more hepatic air, as 6 measures of hepatic unite to 7 of alkaline air; and thus the strength of alkaline liquors, and their real contents, may be

determined better than by any other method. Also the smoaking liquor of BOYLE, which is difficultly prepared in the usual way, may easily be formed by placing the volatile alkali in the middle glass of Dr. NOOTH's apparatus for making artificial mineral waters, and decomposing artificial pyrites, or liver of sulphur, in the lower glass, by marine acid.

Oil of olives absorbs nearly its own bulk of this air, and obtains a greenish tinge from it.

But *new milk* scarcely absorbs one-tenth of its bulk of this air, which is very remarkable, and is not in the least coagulated.

Oil of turpentine also absorbs its own bulk of this air, and even more; but then becomes turbid. Water seems also to precipitate this air from it, for when shaken with it a white cloud appears.

Spirit of wine, whose specific gravity was 0,835, absorbed nearly three times its bulk of this air, and became brown. By this means sulphur may be combined with spirit of wine much more easily than by Count LAURAGAI's method, the only hitherto known. Water precipitates the sulphur in part.

Sulphurated spirit of wine does not tinge litmus red; but it precipitates lime-water, as highly rectified spirit of wine singly does. It also precipitates and gives a brown colour to acetous baro-felenite, which spirit of wine alone also does. It turns the solution of silver black and reddish brown. Concentrated vitriolic acid precipitates the sulphur from it, which neither the nitrous nor marine acids can effect.

When hepatic air is mixed with an equal bulk of *vitriolic æther*, the bulk of the air is at first increased; but afterwards half of it is absorbed, and a slight precipitation appears. The smell of the æther is mixed with that of the hepatic air; but on adding

water

water it becomes very offensive, resembling that of putrefying animal substances.

To one measure of hepatic air I added 1,5 of the *nitrous solution of silver*: the air was immediately, without agitation, reduced to half a measure, and the solution blackened. The remaining air admitted a candle to burn naturally. Hepatic air was also absorbed, but not so readily, nor in such quantity, by the solution of *vitriols of iron and silver*; that of silver was blackened; that of iron at first became white, but by agitation darker. The residuary air burned blue, as hepatic air usually does.

SECTION V.

Of the Properties of Water saturated with Hepatic Air.

This water turns tincture of litmus red.

It does not affect lime-water.

It does not form a cloud in the solution of marine, though it does in that of acetous baro-selenite.

The solutions of other earths in the mineral acids are not altered by it.

When dropped into a solution of *vitriol of iron* or marine salt of *iron*, it produces a white precipitate.

In nitrous salt of *copper* it causes a brown precipitate, and the liquor is changed from blue to green. The precipitate redissolves by agitation. In vitriol of copper it forms a black precipitate.

The solution of *tin* in aqua regia is precipitated by it of a yellowish white colour; that of *gold*, black; that of *regulus of antimony*,

antimony, red and yellow; that of *platina*, red mixed with white.

The solution of *silver* in the nitrous acid, and also that of *lead*, whether in the acetous or nitrous acid, are precipitated black. If the solutions are not perfectly saturated with metal, the precipitates will be brown or reddish brown, and may be redissolved by agitation.

The nitrous solution of *mercury* is precipitated of a yellowish brown; that of sublimated corrosive, yellow mixed with black; but by agitation it becomes white.

The nitrous solution of *bismuth* becomes, by mixture with this water, reddish brown, and even assumes a metallic appearance; that of *cobalt* becomes dark; that of *zinc*, of a dirty white; that of *arsenic*, in the same acid, yellow mixed with red and white, orpiment and realgar being formed.

If *oil of vitriol*, whose specific gravity is 1,863, be dropped into hepatized water, it renders it slightly turbid; but, if the *volatile vitriolic acid* be dropped into it, a bluish white and much denser cloud is formed in the water.

Strong nitrous acid, whether phlogisticated or not, causes a copious white precipitation; but *dilute* nitrous acid produces no change. *Green* nitrous acid, whose specific gravity was 1,328, immediately precipitated sulphur from it.

Strong marine acid produced a slight cloud; but neither distilled vinegar nor acid of sugar had any effect.

It is said by Mr. BERGMAN, that hepatized water in a well closed vessel effects a solution of iron in a few days; but this experiment, on repeated trials, did not succeed with me: nor could I dissolve any other metal in this water; the sulphur indeed unites to many of them, but forms an insoluble mass;

so that, I presume, metallic substances can never be found in hepatized mineral waters.

SECTION VI.

Of the Properties of Alkaline Liquors impregnated with Hepatic Air.

I have already mentioned the proportion of air they are able to take up. Colourless fixed alkaline liquors receive a brownish tinge from this air. The residuum they leave is of the same nature as the part they absorb.

A caustic fixed alkaline liquor, saturated with this air, precipitates *barytes* from the acetous acid, of a yellowish white colour. It also decomposes other earthy solutions, and the colour of the precipitates varies according to their purity, and perhaps this test might be so far improved as to supply the place of the Prussian alkali.

It precipitates the solution of vitriol of *iron*, and also marine salt of iron, black; but this latter generally whitens by agitation. That which I used was very saturate.

The solutions of *silver* and *lead* are also precipitated black with some mixture of white; that of *gold* is also blackened; but that of *platina* becomes brown.

Solutions of *copper* let fall a reddish black or brown precipitate.

Sublimate corrosive by this test discovers a precipitate partly white and black, and partly orange and greenish.

In the nitrous solution of *arsenic* it forms a yellow and orange; and in that of *regulus of antimony*, in aqua regia, an orange precipitate mixed with black.

Nitrous

Nitrous solution of *zinc*, thus treated, shews a dirty white; that of *bismuth* a brown mixed with white; and that of *cobalt* a brown and black precipitate.

As *Prussian alkali* always contains some iron, it gives a purple precipitate with this test, which precipitate is easily dissolved.

It turns tincture of raddishes, which is my test for alkalies, green.

The action of liver of sulphur on metallic substances in the *dry way* is described by many authors, and particularly in an excellent Dissertation by M. ENGESTROM; but its action in the *moist way* has not been mentioned, as far as I recollect, by any. Hence I tried its effect on a few grains of iron, copper, lead, tin, zinc, bismuth, regulus of antimony, and of arsenic. Putting each into a bottle, containing about three half ounces of liquid liver of sulphur, so far diluted that its colour was yellow; in about fifteen days I found they all, except the zinc and tin, had attracted sulphur from the fixed alkali. Iron, arsenic, and regulus of antimony and lead, were most altered; copper next, and bismuth least: but the liquors held none of the metals in solution; that which contained iron became green; on adding an acid sulphur was precipitated; if it held iron it could not at that period be detected.

Water saturated with the condensed residuum of alkaline and hepatic air, that is, with the purest volatile liver of sulphur, does not precipitate marine *selenite*, though it forms a slight brown and white cloud in that of marine *baro-selenite*.

It produces a black precipitate in the solution of vitriol of *iron*, and a black and white in that of marine salt of iron; but by agitation this last becomes wholly white.

It

It precipitates both vitriol of copper, and the nitrous salt of copper, red and brown.

Tin in aqua regia gives a yellowish precipitate; *gold* a dilute yellow and reddish brown; *platina* a flesh-coloured precipitate; and *regulus of antimony* a yellowish red.

Silver is precipitated black; and so is *lead* both from the nitrous and acetous acids.

Sublimate corrosive appeared for an instant red; but soon after its precipitate appeared partly black and partly white.

The nitrous solution of *bismuth* affords also a precipitate, partly black, partly white, and partly reddish brown, and of a metallic appearance; that of *cobalt* is also black or deep brown.

Arsenical solutions give yellow precipitates more or less red; but those of *zinc* only a dirty white.

All these colours vary in some degree, according as the liquors are more or less saturated previous to and after their mixture, and the time they have stood together.

SECTION VII.

Of the Constitution of Hepatic Air.

From an attentive consideration of the above experiments, which I purposely disengaged from all theory, it is difficult to conclude, that hepatic air consists of any thing else than sulphur itself, kept in an ærial state by the matter of heat. Every attempt to extract inflammable air from hepatic air, when drawn from materials that previously contained nothing inflammable, namely, from alkaline or calcareous hepars,

proved abortive: on the contrary, when the materials could previously supply inflammable air, as when martial carbonaceous and saccharine compounds were employed, inflammable air, in ever so small a proportion, was detected: nor could hepatic air be procured from the direct union of inflammable air and sulphur, as we have seen.

Some have imagined, that this air consists of liver of sulphur itself volatilized, and consequently that an alkali enters into its composition; but many weighty reasons oppose this supposition. In the first place this air is evidently, though weakly acid, since it reddens litmus, and precipitates acetous baro-selenite. 2dly, It may be extracted from materials that either contain no alkali at all, or next to none, as iron, sugar, oil, charcoal: and, lastly, it is not decomposed by marine or fixed air, by which, nevertheless, liver of sulphur is decomposable.

I formerly thought that sulphur was held in solution in hepatic air, either by vitriolic or marine air; yet though both of them may hold sulphur in solution, as we have seen, still neither of them is essential to the constitution of hepatic air as such, since it is producible from materials that contain neither of these acids; and, from whatever subject it is obtained, it exhibits the characters of one and the same acid, namely, the *vitriolic exceedingly weakened*; and such an acid we may suppose sulphur itself to be.

In effect, sulphur, even in its concrete state, affords many characters of acidity. It unites with alkalies, calcareous and ponderous earths, and most metals, as a weak acid might: and except a manifest solubility in water (a property which some other concrete acids also possess in a very weak degree) it exhibits every character of acidity. But its acidity is the weakest possible, since it decomposes only acetous, and not marine baro-selenite,

selenite, and is separable from alkalies and earths by all other acids.

That the matter of heat enters into the composition of this air, is evident from the experiments of M. SCHÉELE, who paid particular attention to that object. He found, that acids excite much less sensible heat in uniting with *liver of sulphur*, whether alkaline or calcareous, than while uniting with a proportion of caustic fixed alkali or lime equal to that which enters into the composition of those *livers*; whence he justly infers, that the *difference* enters into the composition of the hepatic air produced. I have proved the same thing another way: for, instead of decomposing an *alkaline* hear by marine acid, I tried to decompose it by a saturate solution both of marine selenite and marine Epsom. The decomposition indeed took place, but no hepatic air was produced: for the acid having given out its specific heat on uniting to the earths, had none to lose or communicate on uniting to the alkali, and consequently the sulphur receiving none could not be thrown into an aërial state.

It is remarkable, that bodies capable of an aërial form receive the latent heat necessary for that form, much more readily from a body that parts with its specific heat than by the mere application of sensible heat. Thus aërated barytes cannot be decomposed by mere heat, as Dr. WITHERING has shewn, though its air is easily separated from it by an acid; and in the same manner antimony cannot be desulphurated even by vitrefaction, though it may by acids: so liver of sulphur will not give hepatic air by mere heat, though it will by the intervention of an acid, even the weakest. The reason of which seems to be this: the matter of heat has no particular affinity with any substance, as is evident from its passing

indifferently from any hot body to a colder, of whatever sort or kind the bodies may be; but it is determined to unite with this or that body in a latent state, in greater or lesser quantity, in proportion to the greater or lesser *capacity* of these bodies to receive it. Now acids, by uniting to the alkaline basis of liver of sulphur, expel the sulphur, and give it their heat, at the *instant* the sulphur, by its separation, has the capacity to receive it; whereas sensible external heat, acting alike on both the constituent parts of liver of sulphur, separates neither; or if it separates them, yet, by its *successive* action, it throws one of them into a *vaporous* state first, and bodies that *first* acquire this state can never after acquire an aerial state by any *subsequent* accession of heat.

The vitriolic and nitrous acids are less adapted to the production of hepatic air than the marine acid, though they contain more specific heat than the mere acid part of the marine acid: the most probable reason of which is, because they have a stronger attraction to sulphur itself, and so detain it.

Hepatic air is much disposed to give out its latent heat, particularly when in contact with substances to which it has any affinity; thus it is condensed in water in a few days; it is also condensed by long exposure to fresh surfaces of mercury or silver or other metals, particularly if they are moist. M. BERGMAN found it in great measure condensed into sulphur, when inclosed alone in a bottle*. In this case it probably contained an excess of sulphur; for hot hepatic air is capable of keeping a farther quantity of sulphur in solution, and depositing it when cold, as I have frequently observed.

* See a note in the second volume of M. MORVEAU's translation of the second volume of BERGMAN's Works, p. 341.

The precipitation of metallic substances by this air is owing partly to the union and phlogification of the acids by this air, and partly to its union with the metals themselves, for it evidently unites in most cases to both.

As alkalies and sulphur are known to have an affinity to each other, we easily understand why hepatic and alkaline airs are condensed when mixed with each other; nor is there any difficulty in conceiving why hepatic air is not condensed by common, dephlogisticated, inflammable, or phlogisticated airs, or remarkably by marine air; but it seems very extraordinary, that hepatic air and vitriolic air should be condensed, and in great measure converted into sulphur by their mutual action on each other, particularly as they both seem nearly of the same species, or at least nearly allied to each other. The attraction of two bodies, thus circumstanced, is certainly very extraordinary; yet that their union proceeds from attraction seems pretty evident, for concentrated vitriolic acid, and particularly volatile vitriolic acid, precipitates sulphur copiously from hepatised water. Volatile vitriolic acid frequently holds some sulphur in solution, as appears from the experiments of Dr. PRIESTLEY and M. BERTHOLLET; and hence it deposits some when it loses its aerial form, or by mere length of time; but the whole of this condensed air is not turned into sulphur, for the water that washed the precipitated sulphur took up a quantity of volatile acid and fixed air, as has been shewn.

The condensation of hepatic air by nitrous air seems owing to the same cause; for when the nitrous air was in great degree deprived of superfluous acid, the condensation of the hepatic was much slower; and that which after all took place seems to have been effected by the decomposition of the nitrous air, and the consequent extrication of an acid.

The

The decompositions effected by fixed and volatile livers of sulphur obviously proceed in most cases from a double affinity.

S E C T I O N VIII.

Of Phosphoric Hepatic Air.

As phosphorus, in respect to its constituent parts, bears a strong resemblance to sulphur, I was naturally led to examine its phenomena when placed in the same circumstances: I therefore gently heated about 10 or 12 grains of phosphorus, mixed with about half an ounce of caustic fixed alkaline solution, in a very small phial, furnished with a bent tube, and received the air over mercury. Upon the first application of heat two small explosions took place, attended with a yellow flame and white smoke, which penetrated through the mercury into the receiver; these were followed by an equable production of air. At last the phosphorus began to swell and froth, and fearing the rupture of the phial, I stopped the tube to prevent the access of atmospheric air, and removed the phial to a water tub, intending to throw it in; but in the mean while the phial burst with a loud explosion, by reason of an obstruction in the tube, and a fierce flame immediately issued from it. However I obtained about 8 cubic inches of air.

This air was diminished very slightly, by agitation with an equal bulk of water, and then became cloudy like white smoke, but soon after recovered its transparency. Upon turning up the mouth of the tube to examine the water, the unabsorbed air instantly took fire, and burned with a yellow flame without exploding, leaving a reddish deposit on the sides of the tube.

Water

Water impregnated with phosphoric air, and over which this air had burned, slightly reddened tincture of litmus :

Did not affect Prussian alkali :

Had no effect on the nitrous solutions of *copper* or *lead*, *zinc* or *cobalt*, nor on marine solution of *iron* or *tin*, or of *tin* in aqua regia, nor on the vitriolic solutions of *iron*, *copper*, *tin*, *lead*, *zinc*, *regulus of antimony*, *arsenic*, or *manganese* ; nor on the marine solutions of *iron*, *copper*, *lead*, *zinc*, *cobalt*, *arsenic*, or *manganese*.

But it precipitated the nitrous solution of *silver* black, and vitriol of *silver* brown ; also nitrous solution of *mercury* made without heat brown and black ; but vitriol of *mercury* first became reddish, and afterwards white ; and sublimate corrosive yellow and red mixed with white.

Gold dissolved in aqua regia is precipitated purplish black, and from the vitriolic acid brownish red and black ; but *regulus of antimony* in aqua regia is precipitated white by this phosphorated water.

The nitrous solution of *bismuth* shewed first a white, and presently after a brown precipitate. Vitriol of *bismuth* and marine salt of *bismuth* were also precipitated brown ; this latter re-dissolved by agitation.

The nitrous solution of *arsenic* also became brown, but re-dissolved by agitation.

I afterwards impregnated some water with this air, without suffering the air to burn over it : it scarcely affected litmus, did not precipitate lime-water ; but it caused a black precipitate in solution of *silver*, a white precipitate of *regulus of antimony* in *R*, and a whitish yellow in that of *sublimate corrosive*.

To a measure of this air I let up a measure of water, and through this some small bubbles of *common air* ; every bubble flamed.

flamed and produced a white smoke until about half as much common air was introduced as there was originally of phosphoric; and yet the original bulk did not appear increased; the flame each time produced a small commotion, and a smoke descended after inflammation into the water: when flame ceased to be produced, smoke still followed the introduction of more common air. Bubbles of phosphoric air, escaping through mercury into the atmosphere, flame, crackle, and smell, exactly like the electric spark*.

To a measure of phosphoric air I let up a half measure of *nitrous air*: a white smoke appeared, with an exceeding slight diminution, and the transparency was soon restored, a slight scum being deposited on the sides of the jar. Another half measure of nitrous air produced no smoke or diminution; but on adding water, and agitating the air in it, much more of it was absorbed. Upon turning up the jar the nitrous air first escaped in the form of a red vapour, and this was followed by a whitish smoke. The water had a phosphoric smell, and precipitated the solution of silver brown. In this experiment the acid of the nitrous air seems to have acted the same part that it does in hepatic air.

Phosphoric air was scarce at all diminished by the addition of an equal measure of *alkaline air*; and water being put up to these, took up in appearance little else than alkaline air, yet on turning up the mouth of the jar, the residuary air smoked without flaming.

* A few months after I made these experiments on phosphoric air, the tenth volume of the *Mémoires des Savans Étrangers* was published; and in this I found, that the spontaneous inflammation of this air was known to M. GINGEMBRE in the year 1783. His experiments are now published in ROZIER'S Journal for October, 1785.

The water, thus impregnated, had exactly the smell of onions. It turned tincture of radishes green.

It precipitated solution of *silver* black; and that of *copper* in the nitrous acid brown; but this precipitate was re-dissolved by agitation, and the liquor became green. *Sublimate corrosive* was precipitated yellow mixed with black.

Iron was precipitated white, both from the vitriolic and marine acids; but a pale yellow solution of it in the nitrous acid was not affected; and a red solution of it in the same acid was only congrumated.

Regulus of antimony in aqua regia gave a white, *cobalt* in nitrous acid a very slight reddish, and *bismuth* in the same acid a brown precipitate.

But neither the nitrous solution of *lead* or *zinc*, nor that of *tin* in marine acid or aqua regia, nor that of *regulus of antimony* in aqua regia, were any way affected.

Fixed air, mixed with an equal proportion of phosphoric air, produced a white smoke, some diminution, and a yellow deposit. On agitating the mixture in water, the fixed air was taken up all to one-tenth. The residuum smoked, but did not inflame spontaneously.

To a small portion of phosphoric air I introduced some *precipitate per se*. It soon grew black, and a white smoke appeared. In two days the precipitate remained solid, yet acquired a shining pale white colour, like that of steel: the air lost its spontaneous inflammability; but I am not certain that this want of inflammability did not proceed from some other cause; for two days after I made this air, I found a quantity of it, which had rested all night on water, had deposited a yellow scum on the sides of the jar, and lost its spontaneous inflammability next morning. The temperature of the air was

then 53° ; and when it inflamed before, the temperature was 68° .

From these few experiments, which the small quantity of air I then obtained did not suffer me to repeat, I think we may conclude, that phosphoric air is nothing else but phosphorus itself in an aërial state, and differs from sulphur in this, among other points, that it requires much less latent heat to throw it into an aërial form, and hence may be disengaged from fixed alkalis, without the assistance of an acid.



VI. *Observations on the Affinities of Substances in Spirit of Wine.*
In a Letter to Richard Kirwan, Esq. F. R. S. by John
Elliot, M. D.

Read January 19, 1786.

S I R,

IN your excellent papers on the attractive powers of the mineral acids, you shew that metallic calces have stronger attractions to those acids, than alkalies and earths. The following experiments not only confirm this doctrine, but also a position that I have lately ventured to advance*, “that certain decompositions will take place in spirit of wine, which “will not at all in water, nor in the *dry* way.”

I have shewn, that if expressed oil be mixed with flaked lime into a paste, so as to form calcareous soap, and mild alkali be added, the latter will not decompose the former, either in water or by fusion. But that if spirit of wine be substituted for water, an alkaline soap and mild calcareous earth will be formed. As sea salt contains the fossil alkali, and as by your table of affinities acids have stronger attraction to metallic calces than to alkalies, I concluded, that if sea salt were added to a metallic soap, a similar double decomposition would take place.

* In an Appendix to the second edition of the “Elements of the Branches of
 “Natural Philosophy connected with Medicine.”

To try this I took some diachylum, which had been bought at Apothecaries-Hall, and added to it sea salt; then covered them to a sufficient height with spirit of wine, and set the bottle over the fire. Soon after they had boiled, the decomposition of the diachylum began to be apparent. When the boiling had continued some time, I removed the vessel from the fire, and after it had stood a few minutes, decanted the clear liquor while hot; then evaporating it, obtained a true alkaline soap. The residuum of course contained a quantity of calx of lead, combined with marine acid.

But much of the diachylum remained either wholly or partly undecomposed: I therefore added more sea salt and spirit of wine, and obtained a further yield of soap. But though much sea salt remained behind, diachylum was still found in the residuum. I found, indeed, that if the ingredients were previously freed from their water, the process succeeded to somewhat better advantage.

From five ounces of diachylum I did not get quite three ounces of soap. This soap was likewise soft, and contained a portion of oil not combined with a sufficient quantity of alkali. The oil, I suppose, had existed in a similar state in the diachylum: and I remarked, that as the spirit evaporated, it gave out the true soap first, the unsaturated oil not till afterwards; so that the latter might easily be obtained separate from the former.

If too much salt was employed, much of it was taken up by the liquid, and communicated to the soap, at least if the ingredients had not been previously deprived of their water. To separate this salt I dissolved the soap in hot water. When the liquor was cold, the soap floated at top, the salt remaining in the water underneath. If too little salt was used, this inconvenience

inconvenience did not happen, or not in so great a degree, though then less soap was of course obtained.

As diachylum, though with a greater proportion of litharge, and boiled longer than that I had from the Hall, still contained oil not sufficiently saturated, I made the metallic soap in another way. To a solution of sugar of lead in water I added a solution of alkaline soap in the same liquid. A double decomposition took place, the oil uniting with the calx of lead, the alkali with the acid of salt. Using this metallic soap instead of the other, I obtained an alkaline soap harder and more perfect than in the preceding process; but still found that part of the oil remained with the calx of lead in the residuum, and adhered so firmly, that repeated quantities of sea salt and spirit of wine did not wholly separate it.

As I have given this process more with a philosophical view than any other, I have been thus particular in my account of it, to shew that however eligible it may appear at first view, it will not answer for making soap for common sale. The alkali indeed is procured much cheaper than from barilla, as the lead may be revived and re-calcined into litharge. But the whole of the oil or fat cannot easily be converted into soap, though in order the better to effect it, I have mixed sand with the diachylum: and as the oil and litharge must, in the large way, be united by boiling, a considerable part of the former will not be sufficiently saturated. Fuel must be used, not only for forming the metallic soap, but likewise for decomposing that soap, and then distilling off the spirit, which will also require additional time and labour. The quantity of spirit of wine lost, though the process (so far as that liquid is concerned) be performed in a still, will alone nearly counterbalance the saving in respect to alkali. And in the process itself there

there is considerable danger, not only of the spirit taking fire from the carelessness of the workmen, but likewise from the frequent explosions that happen during the decomposition of the metallic soap.

As in the experiment with calcareous earth and mild alkali, so in this, I found that the decomposition would not take place when water was used, nor by fusion. In the latter case, I found that the salt was so strongly attracted, that it quitted its water of crystallisation to unite to the metallic soap. If spirit of wine was added to this mass, a double decomposition took place, as already described.

Instead of sea salt, I added to diachylum GLAUBER's salt, freed from its water of crystallisation by heat. I expected that it would have acted on the metallic soap more speedily than the sea salt; but the contrary appeared on trial. On adding a small quantity of sal sodæ, the decomposition went on better, and sufficiently to shew that the ingredients were capable of acting on each other. And I suppose, from your table, that other neutral and earthy salts will have a similar effect, especially if deprived of any superfluous acid by the addition of a little alkali or earth; though I have not made the trials.

Professor BERGMAN has divided his table into two parts; the affinities as they take place in the *moist*, and in the *dry* way. But these experiments shew, that in the *moist* way the affinities take place differently, according as water, or spirit of wine, is used. Perhaps a like difference would be found on using other liquids, each of which would probably afford a different table: for much depends on the attraction which the ingredients themselves have to the liquid employed, as I have endeavoured to shew in the work before referred to; for the liquid is to be considered as one of the ingredients.

I beg leave to add, Sir, that I think I have since hit upon a better method of making soap, and without spirit of wine; but as I have not yet made all the experiments on this subject that I intended, I cannot at present give you an account of them. If they succeed, I will take the liberty to acquaint you with the result.

I am, Sir, with the greatest respect, &c.

J. ELLIOT.

Great Marlborough-Street,

October 31, 1785.

P. S. Since writing the above I have found, that if mild fixed alkali be added to diachylum in hot water, they unite into a gelatinous mass, which is miscible with the water. This may be considered as a kind of *hepar*. If this substance be put into hot spirit of wine, the decomposition already described takes place. If chalk be substituted for alkali, there is a similar result. I have found that nitre is decomposed by diachylum in spirit of wine. I have also found, that if the compound of diachylum and common salt be put into hot spirit of turpentine, the diachylum is dissolved, but the salt remains at the bottom of the vessel.



VII. *An Account of some minute British Shells, either not duly observed, or totally unnoticed by Authors. In a Letter to Sir Joseph Banks, Bart. P. R. S. by the Rev. John Lightfoot, M. A. F. R. S.*

Read January 26, 1786.

DEAR SIR,

AS you were pleased to think a few shells, which I lately submitted to your inspection, might not be unworthy the notice of the Royal Society; encouraged by so respectable an opinion, I shall now beg leave to lay before you some Drawings which I have caused to be made of them, together with such remarks concerning them as may tend, in some degree, to illustrate their natural history.

The first I shall mention is an univalve, coiled up into a spiral form, the cavity of which is divided into three, four, or more distinct chambers or apartments by solid transverse *septa*, which communicate with each other by a *triradiated* aperture.

These characters accord with no genus of shells, hitherto established, so well as the *Nautilus*. It is true, it has not so many chambers as others of that genus, nor are the apertures of the *septa* of a *tubular* form; but as these, according to the laws of method, are to be considered as marks of a *specific* rather than *generic* nature, so I shall not hesitate to refer the shell under consideration to the family of *Nautilus*, at least till

we

we are authorised, by the discovery of many more of a similar structure, to rank it under a new genus.

That I may give a more full and specific description of this singular shell, it must be observed, that its figure is a flattened spiral, umbilicated on one side, convex on the other, but yet slightly depressed in the centre, measuring in diameter about a quarter of an inch; that it is generally coiled up into four volutions, which are convex above, and so nearly plane beneath as to form an acute or carinated margin; and that each of these volutions, on the upper side, has a narrow thread-like border or fillet on the interior edge. The front view of the mouth is obliquely semioval, the upper edge projecting farther than the lower.

The substance of the shell is very brittle and pellucid, and, when alive, of a reddish brown or chestnut colour throughout, except about three or four faint white lines, which appear like rays running from the central umbilicus to different and nearly equidistant parts of the circumference. These white lines are not straight, but shaped each like a short curve, or comma, on the upper side, and are nothing else but the shades of the *septa* in the cavity of the shell.

Such is its *external* appearance. The internal structure is extremely curious; for the whole cavity is divided into three, four, or five chambers or compartments (according to the age of the shell) at nearly equal distances, by transverse *septa* of a hard white brittle semipellucid substance, resembling agate or enamelled glass. Each of these *septa* has a triradiated aperture not unlike the Greek capital upilon, or the Roman Y, inverted, (λ) through which the animal, by means of its soft compressible and extensible nature, easily contrives to extrude

itself, as much as is necessary, when in search of food, or in the act of moving from one place to another.

It may not be amiss here to observe, that the *septa* above-mentioned are totally foreign, both in *use* and *structure*, from what are called *opercula* in other shells: I mean those temporary covers or stoppers, made use of by many testaceous animals to close up the mouths of their shells, and defend them from injury in their quiescent state.

The *opercula*, however various in substance, are always observed to be *single*, *imperforate*, *moveable* at the will of the animal, and constantly placed, as a security, in the *mouth*, never in any *other part* of the cavity of the shell; whereas the *septa*, in the subject now before us, are repeatedly constructed in several parts of the cavity, are all of them *perforated*, intimately connected with the substance of the shell, and consequently *fixed* and *permanent*, as in all the *Nautili*.

And as to the *use* of these *septa*, though I dare not say what might be the real intention of nature in their formation, yet it will be no presumption to affirm, that they could not be designed for the same purpose as *opercula* in other shells; not only because they are placed where they cannot answer the same end, but more especially on account of their open structure, which intirely excludes them from the possibility of affording a proper defence to the enclosed animal.

Should it be said, that they only serve to point out the different *periods* or *stages* of the shell's growth, and are nothing else but the *limits* or *terminations* of the animal's periodical increase, I will not dispute the opinion; it may perhaps be very true; but supposing it to be so, is it not equally probable, that the transverse *septa* in all the *Nautili* are nothing else?

But

But I must not conclude my remarks without taking some notice of the *inhabitant* of this singular shell. It appears to be of the *slug kind*, but differs from the common *land* sorts in this respect, that the *Antennæ* are *filiform*, and the eyes not placed upon their *summits* and *retractile*; but fixed upon the *bead* near their bases, as is probably the case in all the truly *aquatic* kinds, at least in all such as I have hitherto examined. The animal is of a soft and flexible nature, and grey brown colour, and has a power of *extending* itself out of the shell through the aperture of the exterior *septum*; at which time it assumes a *triradiated* shape, not very dissimilar from the aperture itself, or like an inverted Y (λ), the thickest ray of which is the head and body; one of the lines which form the angle is the tail, and the other is a kind of dorsal ligament, which extends from the back of the animal, through one of the rays of the aperture, and through the whole cavity of the shell, and all its *septa*, to the centre, as may be seen by placing the shell between the eye and the light (see fig. 3. Tab. I.).

In the concise LINNÆAN mode of description this shell may be named,

Nautilus (lacustris) testa spirali compressa umbilicata carinata, anfractibus tribus supra convexis contiguis, apertura semiovata, septis triradiato-perforatis.

The Fresh-water Nautilus.

I find no author who has taken any notice of this shell, except Mr. WALKER, who, in his late curious publication on *Minute Shells*, has described it under the name of

Helix lineata dorso convexo umbilicata margine acuto; and has given a figure of it in the same work, Pl. I. fig. 28.

But this ingenious gentleman is free enough to confess, that its *chambered structure* had entirely escaped his notice,

otherwise he would doubtless not have ranked it among the *Helices*.

The place where the shell is to be found, is in deep ditches of clear water, adhering to the roots of *Carices*. It was collected near Upton Church, not far from Eton, in Buckinghamshire, in the spring season. Mr. WALKER reports it to be found on flags in Hornhill Brooks, in Kent, but very rare.

The figures annexed will explain what I have been describing much better than words.

Fig. 1. (Table I.) The shell of its natural size, with the umbilicated side uppermost.

2. The same with the depressed side uppermost; the dark shade in both shewing how far the cavity of the shell is occupied by the dead animal included.

3. The shell magnified with the *depressed* side uppermost, shewing the live animal within it, its head and *antennæ* protruded. Here the white lines appear double, being the shade of the *septa* on both sides of the shell.

5. The same magnified with the *umbilicated* side uppermost, the head and under side of the animal appearing to view.

4. The same magnified in a perpendicular view, with the mouth in front, but cut away down to the first *septum*, in order to shew not only the *carina* or keel of the shell, but more especially the exact appearance of the triradiated *septum* nearest the mouth, and in what manner the animal contrives to extrude itself through the aperture, the head and tail being accommodated to pass through two of the parts of the inverted Y (λ), while the *dorsal ligament* occupies the third.

8. The animal's excrement.

6. 7. Horizontal sections of the shell, in order to shew the internal structure, or the appearance of the *septa*, when the shell

shell is ground down or divided in that direction. Fig. 6. shewing the shell ground away in part, with its umbilicated side uppermost. Fig. 7. the same more deeply and evenly ground, with the depressed or more convex side uppermost.

The *second* shell I shall take notice of has much of the same external face with the preceding, and is nearly of the same size and colour, but materially differs from it in having an uninterrupted cavity from the mouth to the center; that is, *no divided chambers or compartments*. This therefore evidently belongs to the genus of *Helix*.

It is strongly umbilicated on one side, and almost plane on the other, the central wreaths being nearly of equal height, or but slightly depressed, and destitute of that narrow border or fillet mentioned in the preceding shell. It consists most commonly of three volutions, convex on both sides, with an obtusely carinated margin, and semioval mouth.

It may be named,

Helix (fontana) testa compressa obtusè carinata, hinc umbilicata, anfractibus tribus utrinque convexis, apertura semiovata.

Fountain Helix.

The figures here given represent this shell, on both sides, in its natural and magnified state, so that more words to describe it are needless.

Fig. 1. (Tab. II.) The shell of the natural size, with the most convex side uppermost.

2. The same, with the umbilicated side uppermost.

3. The shell magnified, the most convex side uppermost.

4. The same magnified, the umbilicated side uppermost.

I do not find that it has been noticed by any author.

It

It was found in the bottom of a spring of clear water, adhering to the under side of rotten leaves, near Bullstode, in Buckinghamshire, in the month of April. It has also been found in some other clear waters in the same neighbourhood, but not common.

A *third* shell I have to mention is a very minute but curious *Helix* of a subconical form, consisting of about five convex wreaths, gradually diminishing towards the apex. The shell is umbilicated at the base, and the wreaths are transversely surrounded with numerous sharp-edged rings, which are produced in the middle or back of each wreath into a kind of spur, formed of compressed and very tender spines. The mouth is a segment larger than a semicircle, but not round enough to constitute the shell a *Turbo*, to which it is nevertheless nearly allied. The colour of the whole shell is brown.

It may be named,

Helix (spinulosa) testa subconica umbilicata, anfractibus 5 convexis, annulis membranaceis acutis cinctis, dorso spinuloso-carinatis, apertura suborbiculari.

Tender prickly *Helix*.

The figures here given represent this shell in different positions, in its natural and magnified state.

Fig. 1. 2. (Tab. II.) The shell, in different positions, of the natural size.

3. 4. 5. The same magnified.

I know no author who has hitherto noticed it.

It was found near Bullstode, at the foot of pales, upon old bricks and stones, after rainy weather, in June and July.

A fourth

A fourth is a minute shell of the *Turbo* kind.

It strongly resembles the depressed *Helices*; but its circular mouth forbids its being ranked in that *genus*.

It consists of four cylindric or rounded volutions, of nearly equal height on one side, but sunk or umbilicated on the other. These volutions are transversely surrounded with numerous sharp-edged membranaceous rings, which are very fragile and deciduous. The mouth, when perfect, is bordered with a compressed erect margin. The colour of the shell is uniformly brown.

It may be named,

Turbo (helycinus) testa depresso-plana, hinc umbilicata, anfractibus 4 torosis, annulis numerosis acutis membranaceis cinctis.

The fine-ringed *Turbo*.

The figures herewith exhibit both sides of the shell, in its natural and magnified state.

Fig. 1. 2. (Tab. III). The shell, on both sides, of the natural size.

3. 4. The same, on both sides, magnified.

No author, that I know of, has described it.

It was found near Bullstrode, upon bare stones, in the spring season, and at other times in moist weather.

The fifth and last shell I have to mention, is a small thin oblong compressed *Patella*, of a horn colour, about a quarter of an inch long, and one-tenth of an inch wide, having a pointed vertex nearest to the lower end, turned downwards, and leaning to one side.

It may be called,

Patella

Patella (oblonga) testa integerrima oblonga compressa membranacea, vertice mucronato reflexo obliquo.

Oblong fresh-water *Patella*.

It is perfectly distinct from the *Patella lacustris* of LINNÆUS both in shape, and flexure of the vertex, as well as being destitute of radiated streaks.

Fig. 1. 2. 3. and 4. (Tab. III.) The natural size in different attitudes.

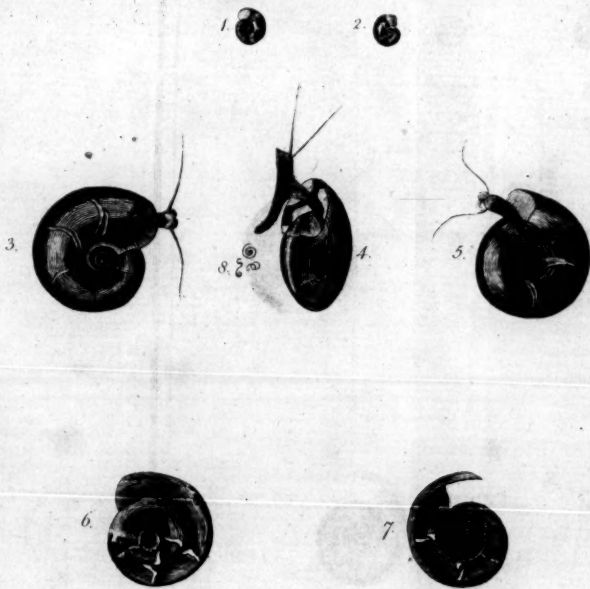
5. A shell magnified, with its vertex upward.

6. *Patella lacustris* LIN. shewing the plan of the two different species.

It has escaped the notice of all the authors I am acquainted with.

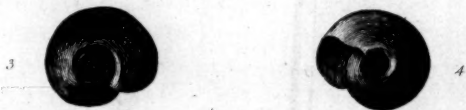
It was found adhering to the leaves of the *Iris Pseudacorus* in waters near Beaconsfield, in Buckinghamshire, by Mr. AGNEW, Gardener to the late Dukes Dowager of PORTLAND; by whose sagacity all the preceding shells were discovered, and by whose faithful pencil they were drawn.

I have now done with describing the shells I intended; but before I conclude, it may not be thought, perhaps, quite foreign to my present subject, to remove, in some degree, an error which has been almost universally adopted by the dealers and collectors in shells, respecting certain subjects, brought from Jamaica, and other parts of the West-Indies, commonly known by the name of *Gold Shells*. They are yellow glossy substances, of an obtusely conical figure, and size of tares or vetch-seeds, composed of several concave brittle imbricated scales, closely compacted, so as to resemble the foliaceous gem or bud of some tree, and have generally a hole or perforation in some part. These are commonly supposed to be *shells*, or the



Nautilus lacustris.

1 2



Helix fontana.

1 2 3



Helix spinulosa.

1 . . . 2

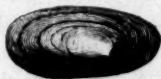


4

Turbo helicina.



5



6



Patella oblonga.

embryos of shells taken out of some bag or ovary. It is certain, however, that this is a mistake; for having collected a few of the largest and most opaque of these *supposed* shells, and such as had no perforation to be found in them, I immersed them for a few minutes in hot water, and then carefully developing the scales of which they were composed, I found in the centre of all the largest and most perfect a small insect, enveloped in a mealy substance, about the size of a small bed-bug, of a roundish oval figure, dark brown colour, convex on the back, slightly concave beneath, and in every instance, except *one* (out of at least fifty which I opened), all without wings. The body was composed of about eight imbricated segments or rings; the head was very short, and almost concealed under the margin of the thorax; however, I plainly discerned, in some of the specimens, that it was furnished with two short filiform *antennæ*. The trunk had six legs; the feet terminated each with a sharp red claw. The body of the *single* specimen which had *wings* was oblong, and narrower than the *apterous* ones. The wings appeared to be glued down to the body, just as in a bee or wasp, when it is almost ready to emerge from the *Pupa* state. Whether they were two or four wings I am not absolutely certain; but they appeared to be of the filmy transparent kind, at least near the extremities; for I clearly perceived the nerves as in the wings of a fly. From hence it evidently appears, that these *Gold Shells* are really no other than the *cases* or cells of an insect in its *Pupa* state; and from considering the *form of its body*, the *difference of the sexes*, the one being *apterous*, the other *winged*, I have no doubt but it is a species of cochineal or *coccus*, and probably one not hitherto described by naturalists. The *cases* do not effervesce with acids, therefore they are not of a *testaceous* nature. They seem to be a vegetable

table substance of the *resinous* kind; for they bubble a little on being burnt on a hot iron, and when triturated dissolve slowly in a warm spirituous menstruum to a sweet-smelling viscid matter. But we must wait for a better elucidation of the subject from those who collect these substances in their native place.

I have the honour to be, with the utmost respect, &c.

JOHN LIGHTFOOT.



VIII. *Observations on the Sulphur Wells at Harrogate, made in July and August, 1785. By the Right Reverend Richard Lord Bishop of Landaff, F. R. S.*

Read February 2, 1786.

IN 1733, when Doctor SHORT first published his Treatise on Mineral Waters, there were only three sulphur wells at Harrogate; there are now four. I made some inquiry respecting the time and occasion of making the fourth well, and received the following account from an old man, who was himself principally concerned in the transaction. About forty years ago, a person who, by lease from the Earl of BURLINGTON, had acquired a right of searching for minerals in the forest of Knaresborough, made a shew as if he had a real intention of digging for coal, on the very spot where the three sulphur wells were situated. This attempt alarmed the apprehensions of the inn-keepers and others at Harrogate, who were interested in the preservation of the wells: they gave him what legal opposition they could, and all the illegal that they durst. At length, for the sum of one hundred pounds, which they raised amongst themselves, the dispute was compromised, and the design, real or pretended of digging for coal was abandoned. Sulphur water, however, had risen up where he had begun to dig. They inclosed the place with a little stone edifice, and putting down a basin, made a fourth well. By a clause in the act of parliament for inclosing Knaresborough Forest, passed

in 1770, it is rendered unlawful for any person whatever to sink any pit, or dig any quarry or mine, whereby the medicinal springs or waters at Harrogate may be damaged or polluted; so that no attempts of the kind above-mentioned need be apprehended in future.

This fourth well is that which is nearest to one of the barns of the Crown-Inn, being about ten yards distant from it. In digging, a few years since, the foundation of that barn, they met with sulphur water in several places. At a very little distance from the four wells there are two others of the same kind; one in the yard of the Half-Moon-Inn, discovered in digging for common water in 1783, and another which breaks out on the side of the rivulet below that Inn. On the banks of that rivulet I saw several other sulphureous springs: they are easily distinguished by the blackness of the earth over which they flow.

On the declivity of a hill, about a quarter of a mile to the west of the sulphur wells at Harrogate, there is a bog which has been formed by the rotting of wood: the earth of the rotten wood is in some places four feet in thickness, and there is a stratum consisting of clay, and small loose decaying sand-stones, every where under it. The hill above is of grit-stone. In this bog there are four more sulphur wells; one at the top, near the rails which separate the bog from the Common; and three at the bottom, though one of these, strictly speaking, is not in the bog but at the side of it in the stratum on which the bog is situated, and at the distance of a yard or two from a rivulet of fresh water, which runs from thence to Low Harrogate, passing close to the side but above the level of the sulphur wells of that place. On the other side of the hill, above the bog, and to the west of it, there is another sulphur well on the side of a brook; and it has been thought that the wells both at Harrogate and in the
bog

bog are supplied from this well. In a low ground, between High Harrogate and Knaresborough, there is a sulphur well; another to the north of it in Bilton Park, at about the distance of a mile; and another to the south of it, at a less distance, was discovered this year in digging for common water by a person of the name of RICHARDSON; and, lastly, there is another at a place called Hookstone Crag: none of these last mentioned wells are above two miles distant from High Harrogate; and by an accurate search a great many more might, probably, be discovered in the neighbourhood.

It is not unusual to dig within a few yards of any of these sulphur wells, and to meet with water which is not sulphureous. I ordered a well to be dug in the fore-mentioned bog, sixteen yards to the south of the sulphur well which is near the rails, and to the same depth with it; the water with which it was presently filled was chalybeate, but in no degree sulphureous. I had another well dug, at about thirty yards distance from the three sulphur wells which are situated at the lower extremity of the bog; this well, by the declivity of the ground, was ten or twelve feet below their level, but its water was not sulphureous. From the first well which I dug, it is evident, that every part of the bog does not yield sulphur water; and from the second, which was sunk into the clay, it is clear that every part of the stratum on which the bog is placed does not yield it, though one of the wells is situated in it.

The sulphur wells at Harrogate are a great many feet below the level of those in the bog; but they communicate with them, if we may rely on what Doctor SHORT has told us — “That about the beginning of this century, when the concourse of people was very great to the Spaw at Harrogate, one ROBERT WARD, an old man, made a bason in the clay under the
the

the moss of a bog where the strongest and briskest of these sulphur springs rise, and gathered half an hoghead of water at a time for the use of the poor; but when he laded this he almost dried the three sulphur wells at the village, whence it is evident, that all have the same origin and communicate with one another." By conversing with some of the oldest and most intelligent people at Harrogate, I could not find that they entertained any opinion of the water at the bog having a communication with that at the spaw. This circumstance might easily be ascertained; and, if the fact should be contrary to what Doctor SHORT supposed, the wells at the bog ought to be covered from the weather as those at the village are; they would by this mean yield great plenty of water for the baths which are wanted by invalids, and which are often very scantily supplied by the wells at Harrogate, notwithstanding the attention which is used in preserving the water which springs at the four wells, by emptying them as often as they become full during both the day and night time. And indeed it is surprising, that the well on the side of the rivulet below the Half-Moon-Inn, which is so well situated for the purpose, has never been inclosed for the furnishing sulphureous water for the baths. The present mode of carrying the water in casks to the several houses where the persons lodge who want to bathe in it, is very troublesome, and the water thereby loses of its virtue. Some of the wells about the village, that for instance which has been discovered at the Half-Moon-Inn, the water of which, I believe, springs from a different source from that which supplies the four sulphur wells, should be either enlarged to a greater horizontal breadth, or sunk to a greater depth, in order to try, by one or both of these ways, whether the quantity and strength of the water might not be increased;

and if that should, as it probably would be the case, one or more baths might be erected after the manner of those at Buxton and other places; or, by proper additional buildings, warm bathing in sulphureous water might be practised, as is done in common water in the bagnios in London. The saltness of the sulphureous water, if that should be thought useful, might easily be made even greater than that of sea water, by adding a quarter of a pound of common salt to every gallon of the water used in forming a bath. The waters at Harrogate, though they have long been very beneficial, have not yet been rendered so useful to mankind, as an intelligent and enterprising person might make them. The alternate strata of sand, stone, and shale, which compose the lower hills near the wells at Harrogate, dip very much, as may be seen in a stone quarry about two hundred yards from the wells; and the same circumstance may be observed in dry weather, in following the bottom of the brook from the village up to the bog; and hence, if there be a communication between the waters of the bog and of the village, as Doctor SHORT asserts, it is probable, that the same stratum of shale which is seen at the bottom of the wells at the village, breaks out again at the bog above the village, and that the water finds its way from the bog to the village through the crevices of that stratum.

After having observed, as carefully as I could, the number and situation of the sulphur wells about Harrogate, I took notice of the temperature of the four at the village. In the month of June, 1780, when the thermometer in the shade was 72° , and the pump water at the Granby-Inn, the well of which is fifty feet deep, was 48° , the strongest of the sulphur wells, being that of which invalids usually drink, was 50° . On the 29th of July in this year, after the earth had been parched with

with drought for many months, the heat of the strongest well was 54° ; the water of the Granby pump was on the same day 48° , and the heat of the air in the shade 76° . Doctor WALKER, who has lately written a treatise on Harrogate water, says, that the heat of this spring was 48° , when that of an adjoining rivulet was 53° . And I have little doubt in believing, that if the experiment was made in cold weather, the temperature of the same well would be found to be several degrees below 48 . This variation of temperature in the sulphur water indicates its springing from no great depth below the surface of the earth; or at least it indicates its having run for a considerable distance in a channel so near to the surface of the earth, as to participate of the changes of temperature, to which that is liable from the action of the sun. But the heat of the sulphur water is not only variable in the same well, at different times, but it is not the same in all the wells at the same time. If we call the strongest well the first, and reckon the rest in order, going to the right, the third well, which is reckoned the next strongest, was 57° hot when the first well was 54° . In support of the conjecture that the sulphur water of the strongest well would in a cold season make the thermometer sink below 48° , which is the constant temperature of springs situated at a great depth in the earth in this country, it may be observed, that though the first and the third well are never frozen, yet the second and the fourth well are frozen in severe weather. When the second and the fourth well are covered with ice, it is probable, that the first and the third have a temperature far below 48° ; but that the sea salt, which is more abundant in them than in the other two wells, and which of all salts resists most powerfully the congelation of the water in which

It is dissolved, preserves them from being frozen in the coldest seasons incident to our climate.

As the temperature of these four wells is not the same in all of them at the same time, nor invariable in any of them, so neither does there seem to be any uniformity or constancy in them, with respect to the quantity of salt which they contain. The salt with which they are all impregnated is of the same kind in all, and it is almost wholly common salt; and though the quantity contained in a definite portion of any one of the wells is not, I think, precisely the same at all seasons of the year, yet the limits within which it varies are not, I apprehend, very great. A method is mentioned in the LXth volume of the Philosophical Transactions, of estimating the quantity of common salt dissolved in water, by taking the specific gravity of the water: this method is not to be relied on, when any considerable portion of any other kind of salt is dissolved along with the sea salt; but it is accurate enough to give a good notion of the quantity contained in the different wells at Harrogate. On the 13th of August, after several days of rainy weather, I took the specific gravities of the four sulphur wells at the village, the drinking well being the first.— Rain water 1.000; first well 1.009; second well 1.002; third well 1.007; fourth well 1.002. By comparing these specific gravities with the table which is given in the LXth volume of the Transactions, it may be gathered, that the water of the first well contained $\frac{1}{71}$ of its weight of common salt; that of the second and fourth, $\frac{1}{114}$; and that of the third, $\frac{1}{122}$. After four days more heavy rain I tried the strongest well again, and found its specific gravity to be 1.008. It is worthy of observation, that the water, as it springs into the first and third well, is quite transparent, but usually of a pearl colour in the second and

fourth, similar in appearance to the water of the first or third well after it has been exposed a few hours to the air; hence it is probable, that the external air has access to the water of the second and fourth well before it springs up into the basin. A great many authors have published accounts of the quantity of common salt contained in a gallon of the water of the strongest well; they differ somewhat from each other, some making it more, others less, than two ounces. These diversities proceed either from the different care and skill used in conducting the experiment; or from a real difference in the quantity of salt with which the water is impregnated at different seasons of the year. The medium quantity of salt contained in a gallon falls short of, I think, rather than exceeds two ounces. The sea water at Scarborough contains about twice as much salt as is found in the strongest sulphur well at Harrogate. The sulphur wells at the bog are commonly said to be sulphureous, but not saline. This, however, is a mistake; they contain salt, and salt of the same kind as the wells at the village. I could not distinguish the kind of salt by the method in which I had estimated the quantity contained in the sulphur wells; I therefore evaporated a gallon of the water of the well in the bog which is near the rails, and obtained a full ounce of common salt, of a brownish colour: the colour would have gone off by calcination. In what degree the medicinal powers of Harrogate water depend on its sulphureous, and in what degree on its saline impregnation, are questions which I meddle not with: I would only just observe on this head, that any strong sulphureous water, such as that of Kettlestone in Derbyshire, or of Shap in Westmoreland, which naturally contains little or no sea salt, may be rendered similar to Harrogate water, by dissolving in it a proper proportion of common salt. The four
sulphur

sulphur wells at Harrogate are very near to each other; they might all be included within the circumference of a circle of seven or eight yards in diameter; yet, from what has been said it is evident, that they have not all either the same temperature, or the same quantity of saline impregnation. This diversity of quality, in wells which have a proximity of situation, is no uncommon phenomenon; and though at the first view it seems to be surprising, yet it ceases to be so on reflexion; for the waters which feed wells so circumstanced, may flow through strata of different qualities situated at different depths, though in the same direction; or through strata placed both at different depths, and in different directions; and that this is the case at Harrogate is probable enough, there being hills on every side of the hollow in which the village is placed.

With respect to the sulphureous impregnation of these waters, I made the following observations.

The inside of the basin, into which the water of the strongest well rises, is covered with a whitish pellicle, which may be easily scraped off from the grit-stone of which the basin is made. I observed, in the year 1780, that this pellicle on a hot iron burned with the flame and smell of sulphur. I this year repeated the experiment with the same success; the substance should be gently dried before it is put on the iron. I would further observe, that the sulphur is but a small part of the substance which is scraped off. That I might be certain of the possibility of obtaining true palpable sulphur from what is scraped off from the basin, and at the same time give some guess at the quantity of sulphur contained in it, I took three or four ounces of it, and having washed it well, and dried it thoroughly by a gentle heat, I put two ounces into a clean glass retort, and sublimed from it about two or three grains of

yellow sulphur. This sulphur, which stuck to the neck of the retort, had an oily appearance; and the retort, when opened, had not only the smell of the volatile sulphureous acid, which usually accompanies the sublimation of sulphur, but it had also the strong empyreumatic smell which peculiarly appertains to burnt oils; and it retained this smell for several days. It has been remarked before, that the salt separable from the sulphur water was of a brownish colour; and others, who have analysed this water, have met with a brown substance, which they knew not what to make of; both which appearances may be attributed to the oil, the existence of which was rendered so manifest by the sublimation here mentioned. I will not trouble the Society with any conjectures concerning the origin of this oil, or the medium of its combination with water; the discovery of it gave me some pleasure, as it seemed to add a degree of probability to what I had said concerning the nature of the air with which, in one of my Chemical Essays, I had supposed Harrogate water to be impregnated. I will again take the liberty of repeating the query which I there proposed. "Does this air, and the inflammable air separable from some metallic substances, consist of *oleaginous* particles in an elastic state?" When I ventured to conjecture, in the Essay alluded to, that sulphureous waters received their impregnation from air of a particular kind, I did not know that Professor BERGMAN had advanced the same opinion, and denominated that species of air, Hepatic Air. I have since then seen his works, and very readily give up to him not only the priority of the discovery, but the merit of prosecuting it. And though what he has said concerning the manner of precipitating sulphur from these waters can leave no doubt in the mind of any chemist concerning the actual existence of sulphur

in them; yet I will proceed to the mention of some other obvious experiments on the Harrogate water, in support of the same doctrine.

Knowing that, in the baths of Aix-la-Chapelle, sulphur is found sticking to the sides and top of the channel in which the sulphureous water is conveyed, I examined with great attention the sides of the little stone building which is raised over the basin of the strongest well, and saw them in some places of a yellowish colour: this I thought proceeded from a species of yellow moss, commonly found on grit-stone: I collected, however, what I could of it by brushing the sides of the building, at the distance of three or four feet from the water in the basin: on putting what I had brushed off on a hot iron, I found that it consisted principally of particles of grit-stone, evidently however mixed with particles of sulphur.

Much of the sulphureous water is used for baths at Harrogate; and for that purpose all the four wells are frequently emptied into large tubs containing many gallons apiece; these constantly stand at the wells, and the casks, in which the water is carried to the several houses, are filled from them. On examining the insides of these tubs, I found them covered, as if painted, with a whitish pellicle. I scraped off a part of this pellicle: it was no longer soluble in water; but, being put on a hot iron, it appeared to consist almost wholly of sulphur. Some of these tubs have been in use many years, and the adhering crust is thick in proportion to the time they have been applied to the purpose; but the sulphur pellicle was sufficiently observable on one which was new in the beginning of this season. The water when it is first put into these tubs is transparent; when it has been exposed to the air for a few hours, it becomes milky; and, where the quantity is large, a white cloud

cloud may be seen slowly precipitating itself to the bottom: This white precipitate consists partly, I am not certain that it consists wholly, of sulphur; and the sulphur is as really contained in the waters denominated sulphureous, as iron is contained in certain sorts of chalybeate waters; in the one case the iron is rendered soluble in water by its being united to fixed air, or some other volatile principle; and in the other sulphur is rendered soluble in water by its being united to fixed air, or some other volatile principle: neither iron nor sulphur are of themselves soluble in water, but each of them, being reduced into the form of a salt by an union with some other substances, becomes soluble in water, and remains dissolved in it, till that other substance either escapes into the air, or becomes combined with some other body.

About forty years ago, they took up the basin of the third well, and a credible person, who was himself present at the operation, informed me, that in all the crevices of the stone on which the basin rested, there were layers of pure yellow sulphur. This I can well believe, for I ordered a piece of shale to be broken off from the bottom of the fourth well; it was split, as shale generally is, into several thin pieces, and was covered with a whitish crust. Being laid on a hot iron, in a dark room, it cracked very much, and exhibited a blue flame and sulphureous smell.

If the water happens to stand a few days in any of the wells, without being disturbed, there is found at the bottom a black sediment; this black sediment also marks the course of the water which flows from the well, and it may be esteemed characteristic of a sulphur water. The surface of the water also, when it is not stirred for some time, is covered with a whitish scum. Doctor SHORT had long ago observed, that
both

both the black Yediment, and the white Scum, gave clear indications, on a hot iron, of their containing sulphur: I know not whence it has come that his accuracy has been questioned in this point; certain I am, that on the repetition of his experiments I found them true. The white scum also, which is found sticking on the grafs over which the water flows, being gently dried, burns with the flame and smell of sulphur. From what has been said it is clear, that sulphur is found at Harrogate, sticking to the basin into which the water springs; sublimed upon the stones which compose the edifice surrounding the well; adhering to the sides of the tubs in which the water stands; subsiding to the bottom of the channel in which the water runs; and covering the surface of the earth, and of the blades of grafs, over which it flows. It is unnecessary to add another word on this subject; it remains that I risk a conjecture or two, on the primary cause of the sulphureous impregnation observable in these waters.

In the Chemical Essay before referred to, I have shewn, that the air separable from the lead ore of Derbyshire, or from Black-Jack, by solution in the acid of vitriol, impregnates common water with the sulphureous smell of Harrogate water; and I have also shewn that the bladder fucus or sea-wrack, by being calcined to a certain point, and put into water, not only gives the water a brackish taste, but communicates to it, without injuring its transparency, the smell, taste, and other properties of Harrogate water. Professor BERGMAN impregnated water with a sulphureous taste and smell, by means of air separated by the vitriolic acid from *hepar sulphuris*, made by fusion of equal weights of sulphur and pot-ashes, and from a mass made of three parts of iron filings melted with two of sulphur; and he found also, that Black-Jack and native Siberian

iron yielded hepatic air, by solution in acids. This, I believe, is the main of what is known by chemists on this subject; what I have to suggest, relative to the Harrogate waters in particular, may perhaps be of use to future inquirers.

I have been told, that on breaking into an old coal-work, in which a considerable quantity of wood had been left rotting for a long time, there issued out a great quantity of water smelling like Harrogate water, and leaving, as that water does, a white scum on the earth over which it passed. On opening a well of common water, in which there was found a log of rotten wood, an observant physician assured me; that he had perceived a strong and distinct smell of Harrogate water. Dr. DARWIN, in his ingenious Account of an artificial Spring of Water, published in the first part of the LXXVth volume of the Philosophical Transactions, mentions his having perceived a slight sulphureous smell and taste in the water of a well which had been sunk in a black, loose, moist earth, which appeared to have been very lately a morass, but which is now covered with houses built upon piles. In the bog or morass above-mentioned there is great plenty of sulphureous water, which seems to spring from the earth of the rotten wood of which that bog consists. These facts are not sufficient to make us certain, that rotten wood is efficacious in impregnating water with a sulphureous smell; because there are many bogs in every part of the world, in which no sulphureous water has ever been discovered. Nor, on the other hand, are they to be rejected as of no use in the inquiry; because wood, at a particular period of its putrefaction, or when situated at a particular depth, or when incumbent on a soil of a particular kind, may give an impregnation to water, which the same wood, under different circumstances, would not give.

The bilge water, usually found at the bottom of ships which are foul, is said to smell like Harrogate water: I at first supposed, that it had acquired this smell in consequence of becoming putrid in contact with the timber on which it rested, and this circumstance I considered as a notable support to the conjecture I had formed of rotten wood being, under certain circumstances, instrumental in generating the smell of Harrogate water. But this notion is not well founded; for the bilge water is, I suppose, salt water; and Dr. SHORT says, that sea water, which had been kept in a stone bottle six weeks “stunk not much short of Harrogate sulphur water.” It has been remarked above, that calcined sea-wrack, which contains a great deal of sea salt, exhales an odour similar in all respects to that of Harrogate water; and in confirmation of the truth of this remark, I find that an author, quoted by Dr. SHORT, says, that “Bay salt thrice calcined, dissolved in water, gives exactly the odour of the sulphur Well at Harrogate.” From these experiments considered together, it may, perhaps, be inferred, that common salt communicates a sulphureous smell to water both by putrefaction and calcination. Hence some may think, that there is some probability in the supposition, that either a calcined stratum of common salt, or a putrescent salt spring, may contribute to the production of the sulphureous smell of Harrogate water; especially as these waters are largely impregnated with common salt. However, as neither the salt in sea water, nor that of calcined sea-wrack, nor calcined bay salt, are any of them absolutely free from the admixture of bodies containing the vitriolic acid, a doubt still remains, whether the sulphureous exhalation, here spoken of, can be generated from substances in which the vitriolic acid does not exist.

The shale from which alum is made, when it is first dug out of the earth, gives no impregnation to water; but by exposure to air and moisture its principles are loosened, it shivers into pieces, and finally moulders into a kind of clay, which has an aluminous taste. Alum is an earthy salt resulting from an union of the acid of sulphur with pure clay; and hence we are sure, that shale, when decomposed by the air, contains the acid of sulphur; and from its oily black appearance, and especially from its being inflammable, we are equally certain that it contains phlogiston, the other constituent part of sulphur. And indeed pyritous substances, or combinations of sulphur and iron, enter into the composition of many, probably of all sorts of shale, though the particles of the pyrites may not be large enough to be seen in some of them; and if this be admitted, then we need be at no loss to account for the bits of sulphur, which are sublimed to the top of the heaps of shale, when they calcine large quantities of it for the purpose of making alum: nor need we have any difficulty in admitting, that a phlogistic vapour must be discharged from shale, when it is decomposed by the air. Dr. SHORT says, that he burned a piece of aluminous shale for half an hour in an open fire; he then powdered and infused it in common water, and the water sent forth a most intolerable sulphureous smell, the very same with Harrogate water. He burned several other pieces of shale, but none of them stunk so strong as the first. This difference may be attributed, either to the different qualities of the different pieces of shale which he tried, or to the calcination of the first being pushed to a certain definite degree; for the combination of the principles on which the smell depends may be produced by one degree of heat, and destroyed by another. I have mentioned, briefly,

briefly, these properties of shale, because there is a stratum of shale extended over all the country in the neighbourhood of Harrogate; several beds of it may be seen in the stone quarry above the sulphur wells; many of the brooks about Harrogate run upon shale, and the sulphur wells spring out of it. They have bored to the depth of twenty yards into this shale, in different places, in search of coal, but have never penetrated through it. Its hardness is not the same at all depths. Some of it will strike fire, as a pyrites does, with steel; and other beds of it are soft, as if in a state of decomposition, and the sulphur water is thought to rise out of that shale which is in the softest state. But whatever impregnation shale when calcined, or otherwise decomposed to a particular degree, may give to the water which passes over it, it must not be concluded, that shale in general gives water a sulphureous impregnation; since there are many springs, in various parts of England, arising out of shale, in which no such impregnation is observed.

I forgot to mention, in its proper place, that having visited the bog, so often spoken of, after a long series of very dry weather, I found its surface, where there was no grass, quite covered over with a yellowish crust, of tolerable consistency, which had a strong aluminous taste, and the smell of honey. BERGMAN speaks of a turf found at Helsingberg in Scania, consisting of the roots of vegetables, which was often covered with a pyritous cuticle, which, when elixated, yielded alum; and I make no doubt, that the Harrogate morass is of the same kind.

Whether nature uses any of the methods which I have mentioned of producing the air by which sulphureous waters are impregnated, may be much questioned; it is of use, however, to record the experiments by which her productions may be

imitated; for though the line of human understanding will never fathom the depths of divine wisdom, displayed in the formation of this little globe which we inhabit; yet the impulse of attempting an investigation of the works of God is irresistible; and every physical truth which we discover, every little approach which we make towards a comprehension of the mode of his operation, gives to a mind of any piety the most pure and sublime satisfaction.



IX. *Observations and Remarks on those Stars which the Astronomers of the last Century suspected to be changeable.* By Edward Pigott, Esq.; communicated by Sir Henry C. Englefield, Bart. F. R. S. and A. S.

Read February 9, 1786.

IT is about a century since HEVELIUS, MONTANARI, FLAMSTEED, MARALDI, and CASSINI, noticed a certain number of stars which they supposed had either disappeared, changed in brightness, or were new ones; and yet to this day we have acquired no further knowledge of them. This may be attributed to the difficulty of finding out what star is meant, and the not having exact observations of their relative brightness. I therefore have drawn up the following catalogue, and made the necessary observations; so that in future we can examine them without much trouble, and be certain of any change that may take place. To accomplish this, it was requisite to compare with attention many authors and most of the catalogues of stars; in doing which I have perceived several undoubted errors, and others highly probable; but as entering into a discussion of such disagreements would swell this account considerably, and make it very intricate, I shall avoid, as much as possible, any thing of the kind that is not immediately necessary.

In order to separate certainty from doubt, I have classed these stars in two divisions; the first are undoubtedly changeable; the

the others remain yet to be better authenticated. Though some of them bear all the appearance of being variable, still no *certainty* of their being so has come to my knowledge. To those of the first class are subjoined observations made on them within these last four years, from which the period and progressive changes of some are deduced, though never settled before, and if already known are more exactly determined by comparing my observations with former ones. Also, as the position of several are determined only by ancient astronomers, and therefore inaccurately, I have observed them with great exactness, the declinations being taken with a BIRD's eighteen-inch quadrant, and the right ascensions with a three-feet transit instrument: these last may serve in future to discover their proper motions in right ascension, for which reason I shall specify the stars to which they were compared. The stars of the second class have either their relative brightness exactly settled, or their non-existence ascertained. I have also pointed out the probability of a mistake in several, and in general given an account of the appearance they have had within these few years.

Catalogue of variable Stars, reduced to the beginning of 1786.

Class the first.

Names.	R. A. in time.	Declination.	Greatest and least magnitudes.	From whence re- duced.
	h. ' "	° ' "		
Nova 1572, in Cassio- pea, ...	00 13 00	62 58 + N	1 - 0	{ Ricciolus's Al- magestum, &c.
• Ceti	2 8 33	3 57 25 S	2 - 0	BRADLEY.
Algol	2 54 19	40 6 55 N	2 - 4	MAYER.
MAYER's 420th in Leo	9 36 5	12 25 00 N	6 - 0	MAYER.
In Hydra	13 18 4+	22 9 38 S	4 - 0	From my observat.
Nova 1604 in Ser- pentarius	17 18 00	21 10 ½ S	1 - 0	Phil. Tr. N° 346.
• Lyre	18 42 11	33 7 46 N	3 - 4.5	BRADLEY.
Near the Swan's head	19 38 58	26 48 ½ N	3 - 0	Phil. Tr. N° 65.
• Antinoi	19 41 34	00 28 14 N	3.4 - 5	LA CAILLE.
In the Swan's neck	19 42 21	32 22 58 N	5 - 0	From my observat.
In the Swan's breast	20 9 54	37 22 37 N	3 - 0	From my observat.
• Cephei	22 21 00	57 20 00 N	4.3 - 4.5	FLAMSTEED.

Class the second.

HEVELIUS's 6 Cassiopeæ	00 23 16	60 50 00 N	7 - 0	HEVELIUS.
46 or • Andromedæ	1 9 46	44 24 00 N	4.5 - 5.6	FLAMSTEED.
50 or • Andromedæ	1 24 16	40 20 15 N	4.5 - 0	FLAMSTEED.
HEVELIUS's 41 Androm.	1 28 40	41 31 ½ N	5 - .	HEVELIUS.
TYCHO's 20th Ceti	1 39 .	13 20 . S	5 - 0	TYCHO.
55 or Neb. Andromedæ	1 40 30	39 40 3 N	6 - .	FLAMSTEED.
PROL. and UL. BEIGH • Eridani	2 42 .	9 40 . S	4 - 0	UL. BEIGH.
41 Tauri	3 53 27	27 00 39 N	5 - 0	FLAMSTEED.
47 Eridani	4 23 54	8 41 40 S	4 - 0	FLAMSTEED.
Near 53d Eridani . . .	4 29 00	12 30 ± S	4 - 0	By estimation.
• Canis Majoris.	6 54 5	15 19 36 S	3 - 0	LA CAILLE.
• Geminorum	7 32 11+	28 31 38 N	1 - 3	MASKELYNE.
• Leonis	9 20 24	12 14 23 N	4 - 6	MAYER.
• Leonis	9 32 3	14 59 36 N	5.6 - 0	MAYER.
25th Leonis	9 46 8	12 20 36 N	6.7 - 0	FLAMSTEED.
BAYER's • Leonis . . .	9 52 ½	15 30 . N	6 - 0	TYCHO.
• Ursæ Majoris . . .	12 4 45	58 13 24 N	2 - 4	LA CAILLE.

Class the second continued.

Names.	R. A. in Time.	Declination.	Greatest and least magnitudes.	From whence reduced.
<i>h. ' "</i>	<i>m. s. "</i>	<i>d. m. s. "</i>		
α Virginis . . .	12 7 43	00 24 16 N	6 0	MAYER.
BAVER's * near γ	12 53 00	10 00 . S	6-0	From maps.
In N. thigh of Virgo	13 29 +	00 30 . S	6-0	From maps.
η Virginis . . .	13 43 43	2 5 50 N	6-0	FLAMSTEED.
δ Draconis . . .	13 58 36	65 24 8 N	2-4	BRADLEY.
In west scales of Libra	14 53 $\frac{1}{2}$	13 26 . S	6-0	{ Mém. de l'Acad. des Sciences.
PROL. and UL. BEIGH's 6th unformed in Libra	15 29 +	20 30 . S	4-7	UL. BEIGH.
α Libræ . . .	15 29 39	19 58 27 S	4-	LA CAILLE.
TYCHO's 11th Libræ	15 37 $\frac{1}{2}$	19 30 . S	4-0	TYCHO.
33 Serpentis . . .	15 38 00	17 14 00 N	6-0	FLAMSTEED.
Near γ Ursæ Minoris	16 $\frac{1}{2}$.	82 $\frac{1}{2}$. N	6-0	From maps.
PROL. 14 Ophiuchi	17 2 14	26 15 37 S	4-0	BRADLEY.
PROL. 13 Ophiuchi	17 18 +	20 35 . S	4-0	PTOL.
PROL. 18 Ophiuchi	17 22 .	24 10 . S	5-0	PTOL.
σ Sagittarii . . .	18 42 00	26 32 34 S	2-4	MAYER.
θ Serpentis . . .	18 45 35	3 56 26 N	4-5	LA CAILLE.
TYCHO's 27th Capricor.	21 41 .	14 28 . S	6-0	TYCHO.
TYCHO's 22d Androm.	21 43 $\frac{1}{2}$	49 15 . N	4-0	TYCHO.
TYCHO's 19 Aquarii	22 25 .	15 55 . S	6-0	TYCHO.
ϵ Andromedæ . . .	22 52 6	41 10 45 N	4-6	LA CAILLE.
LA CAILLE's 483 } Zodi. Cat. . . }	22 55 40	8 50 45 S	7-0	LA CAILLE.

I shall now proceed to give a short account of these stars, and begin with those of the first class.

The famous Nova of 1572 in Cassiopea.

Several astronomers are of opinion, that it has a periodical return, which KEILL and others have conjectured to happen every 150 years. This is also my opinion; and I cannot think its not being noticed at the completion of every term a material

rial objection, since perhaps, as with most of the variables, it may at different periods have different degrees of lustre, so as sometimes to increase only to the ninth magnitude; and if this be the case, its period is probably much shorter. This induced me, in September 1782, to take a plan of the smallest stars near its place, and which I have examined often since, but found no alteration.

o Ceti.

Since the end of 1782 I have observed very exactly the decrease of brightness of this star; but never have seen it of above the 6th magnitude. Oct. 29, 1782, it was of the 7th magnitude, and gradually decreased till Dec. 30, it being then of the 8.9th magnitude.

1783, Feb. 16, certainly less than the 9th magnitude.

1783, August 25, of the 6th magnitude, and gradually decreased until Dec. 14, being then of the 10th magnitude, and equal to the little star close to it.

1784, Jan. 11, I thought it by intervals still less than the same little star.

1784, Sept. 12, it was of the 7.8th magnitude, and gradually decreased until Dec. 9, and then was of the 9th magnitude, and rather brighter than the little star.

As a matter of curiosity, I have deduced its period from the times when it was equal to a certain star in the course of its decrease; the results were 320—337 and 328 days; but M. CASSINI determined its mean period with greater exactness to be 334 days. Mr. GOODRICKE saw it Aug. 9, 1782, of the 2d magnitude, rather brighter than α and less than β Ceti. Sept. 5, it was of the 3d magnitude, being equal to γ Ceti.

Algol.

The period of Algol, discovered by Mr. GOODRICK, gave us some new light into the nature of the fixed stars. Though the phænomena seem to attract the attention of most astronomers, still there are some points which require further investigation. Its degree of brightness, when at its *minimum*, is different in different periods; and also, I think, when at its full, it is sometimes brighter than α Persei, and at other times less. Whether these differences return regularly after a certain number of periods remains yet to be examined. My last observations, when it was at the middle of its *minimum*, are,

- h.
- 1785, July 8, at 11 50 undoubtedly less than ϵ Persei.
 — July 31, at 9 50 equal to ϵ Persei.
 — Sept. 12, at 10 45 { a remarkable observation; rather less than θ Persei;
 { evidently brighter than ϵ ; nearly of the 3d mag.

MAYER's N° 420, lately discovered to be variable by M. KOCH.

A few years before 1782, M. KOCH saw the N° 420 undoubtedly less than the N° 419 of MAYER's Catalogue.

In February 1782, he found them both exactly of the same brightness, therefore of the 7th magnitude.

From an extract of a letter I have lately seen, the variable was of the 9th magnitude in April, 1783, and of the 10th in April, 1784.

I have often seen the N° 419, but never the variable, though I have frequently looked for it with a night-glass, and on the 4th of April, 1785, in a 3-feet achromatic transit instrument.

Variable

Variable in Hydra.

MARALDI, in 1704, having found that this star had a periodical variation, continued to examine it for several years, and concluded its period to be about two years, though with considerable variations; in which he was much mistaken, as will appear from the following results, which shew that its period in all probability is tolerably regular, and only of 494 days.

Dates when it was at the middle of its greatest brightness, estimated from MARALDI's observations.

1704, March 14, he saw it nearly of the same magnitude from the beginning of March until the beginning of April; it then decreased.

1705, . . . he saw it very faint in November, 1705, and found it decreasing: this observation is too imperfect.

1708, May 22, accurately determined; its increase and decrease being well observed.

1709, Nov. 10, :: doubtful, its decrease only being observed.

1712, May 1, :: ditto, ditto, ditto.

1784, Jan. 26, by me, very accurately, its increase and decrease being observed. See the Observations that conclude this paper.

1785, May 27, ditto, ditto, ditto.

The four greatest intervals of MARALDI's Observations give for single periods in days thus 495—517—480 and 510, the mean being 500½, which is tolerably exact considering how

doubtful the observations of 1709 and 1712 are. My two make it 487 days; but as the interval is only a single period, it may err 10 days; I therefore shall take a mean between the results, which is 494, and proceed on to the following comparisons of MARALDI's two best observations with mine.

1708, May 22, } interval of 56 periods, each of $493\frac{1}{2}$ days.
 1784, Jan. 26, }

1708, May 22, } interval of 57 periods, each of $493\frac{1}{2}$ days.
 1785, May 27, }

1704, Mar. 14, } interval of 59 periods, each of $494\frac{1}{2}$ days.
 1784, Jan. 26, }

1704, Mar. 14, } interval of 60 periods, each of $494\frac{1}{2}$ days.
 1785, May 27, }

A single period, on a mean, $\frac{494}{\text{days.}}$

If MARALDI's observations of 1704 and 1708 are exact to a month, and there is no reason to believe otherwise, the period at that time seems to have been a few days longer than it is at present, and therefore the one here deduced may be esteemed as the mean period.

Particulars of the changes it undergoes.

1. When at its full brightness it is of the 4th magnitude, and has no perceptible change for about a fortnight.

2. It is about six months in increasing from the 10th magnitude, and returning to the same.

3. Therefore it may be considered as invisible also during six months.

4. It is considerably quicker in increasing than in decreasing, perhaps by half.

Though

Though when at its full it may always be filed of the 4th magnitude, it does not constantly attain exactly the same degree of brightness, but the differences are very small, as shewn below.

1704, brighter than $\downarrow$.

1708, brighter than $\downarrow$.

1784, { much brighter than $\downarrow$, being nearly
between $\downarrow$ and γ Hydræ.

1785, rather brighter than $\downarrow$.

Its mean right ascension, computed from my observations, and reduced to Jan. 1, 1784, is

199 29 30 { from 4 observations, compared to ζ π , made between March
and May 1784.

199 29 21 from 2 ditto, compared to MAYER's 538, made in May, 1784.

199 29 20 from 5 ditto, compared to γ Hydræ, made between March and May, 1784.

199 29 24 — mean right ascension for Jan. 1, 1784, on a mean.

HEVELIUS's 30th Hydræ is the above star; he marks it of the 6th magnitude; I find it in no other Catalogue.

The famous Nova of 1604, in Serpentarius.

A full account of this star is given by KEPLER, and it seems to have had a similar appearance to the Nova in Cassiopea; therefore the reflections delivered there need not be again repeated. In July, 1782, I took a plan of the smallest stars near its place, which was examined every year since, but no alteration was perceived.

β Lyra.

Mr. GOODRICKE discovered the variation and period of this star, and hopes soon to settle its different phases with more exactness;

exactness; I shall therefore not enter into any detail, being certain it cannot be in better hands. In his last account he mentions having first suspected the period to be only of six days nine hours; such has always been my opinion, and which material point will probably be more satisfactorily determined in his next publication.

Nova near the Swan's Head of 1670.

This star was first seen in December 1669 by Don ANTHELME; it soon became of the 3d magnitude, and disappeared in 1672, after having undergone several variations. I have constantly looked for it since November, 1781, without success; had it increased to only the 10th or 11th magnitude, I should have perceived it, having taken an exact plan of all the surrounding stars.

★ Antinoi.

The variation and period of this star I discovered last year, and had the honour of communicating an account of it to the Society: as at present a long interval is elapsed since my first observations, and that lately I have noted some of its phases with exactness, I shall compare them to those observed in 1784, which of course will give results more satisfactory. The period, as settled in my former paper, is 7 d. 4 h. 38'; but for reasons there alledged, it must be much less precise than the following.

1785, July 18, at 9 h.	} times when ★ Antinoi was between its least and greatest brightness.
Sept. 6, at 18	
Sept. 27, at 22	

These being compared to similar observations of Sept. 12 and 19, 1784, make the length of a single period thus:

D. H. M.

7 4 12

7 4 19

7 4 17

7 4 22

7 4 7

7 4 12

7 4 15 on a mean.

I see no reason to alter materially the other points; but believe them more exact thus:

40 hours at its greatest brightness.

66 — in decreasing.

30 — at its least.

36 — in increasing.

It also, in every period, seems to attain the same degree of brightness when at its full, and to be equally decreased.

Variable in the Swan's Neck.

During these three years I have observed this star with particular attention, as may be seen by the observations that conclude this Paper, and determined the middle time of its greatest brightness very exactly, thus:

1783, July 9, of the 6 . 7th magnitude.

1784, Aug. 4, of the 5 . 6th ———

1785, Sept. 1, of the 6th ———

The second of these, being compared to that of Nov. 20, 1687, made by KIRCH, gives 406 days exactly for one period, the interval between them being 35322 days, and divided by 87 periods. I make the divisor 87, in order to get a result nearest to that settled by MARALDI and CASSINI of 405, and by M. LE GENTIL of 405,3 days. We cannot suppose that these great astronomers have made any mistake; and on the other hand, it seems hardly possible, that the mean of my observations alone, which makes the period 392 days, can err 14; but perhaps its period is irregular; to determine which several intervals of 15 years ought to be taken, and I am much inclined to believe, that it will be found of only 396 days 21 hours.

Particulars of the changes it undergoes.

1. When at its full brightness it has no perceptible change for about a fortnight.
2. It is about $3\frac{1}{2}$ months in increasing from the 11th magnitude to its full brightness, and the same in decreasing.
3. Therefore it may be considered as invisible during six months.
4. It does not attain the same degree of brightness at every period, being sometimes of the 5th, and other times of the 7th magnitude.

Its mean right ascension, computed from my observations, and reduced to Aug. 1, 1783, is

295	33	46	from 2 observ. compared to γ Cygni, made in July and August, 1783.
295	33	45	from 2 ditto, compared to γ Lyrae, ditto.
295	33	45	from 1 ditto, compared to α Lyrae, made in August, 1783.
295	33	55	from 3 ditto, compared to β Lyrae, made in July and August, 1783.
295	33	52	from 2 ditto, compared to β Cygni, made in August 1783.
295	33	48 $\frac{1}{2}$	mean right ascension for August 1, 1783, on a mean.

Variable in the Swan's Breast.

This star was first seen by G. JANSONIUS in 1600, and afterwards frequently observed by different astronomers, but with intervals of ten or more years, which is probably the reason why no regularity in its changes has yet been deduced. I have examined minutely the observations made in the last century, and shall venture to give the following results.

1. Continues at its full brightness for about five years.
2. Decreases rapidly during two years.
3. Invisible to the naked eye for four years.
4. Increases slowly during seven years.
5. All these changes, or its period, are completed in 18 years.
6. It was at its *minimum* at the end of the year 1663.

It does not always increase to the same degree of brightness, being sometimes of the 3d, and at other times only of the 6th magnitude. I am intirely ignorant whether it is subject to the same changes since this century, having not met with any series of observations on it; but if the above conjectures are exact, it will be at its *minimum* in a very few years. Since November, 1781, I have constantly seen it of the 6th magnitude, being rather less than N° 28, 29, and *m*, and rather brighter than

36 and 40 Cygni. Sometimes I suspect it has rather decreased within these two last years, though in a very small degree.

Its mean right ascension, computed from my observations, and reduced to Sept. 1, 1782, is

302 26 43 from 3 observations, compared to γ Cygni, made in October, 1781.

302 26 46 from 3 ditto, compared to γ Cygni, made in August, 1782.

302 26 52 from 1 ditto, compared to β Aquilæ, made ditto.

302 26 46 from 1 ditto, compared to α Cygni, made ditto.

302 26 39 from 1 ditto, compared to δ Andromedæ, made in October 1781.

302 26 45 mean right ascension for Sept. 1, 1782, on a mean.

FLAMSTEED has this star in his Catalogue; but, I believe, observed it only once.

δ Cephei.

This is the last variable star discovered, and again by Mr. GOODRICKE. Its changes are very difficult to be seen, unless examined when at its *minimum* and full brightness. I have lately made some good observations on it thus:

1785, Aug. 30, at 14 h. less than ϵ Cephei.

31, at 9 h. equal if not brighter than ζ Cephei.

Sept. 15, at 12½ h. less than ϵ Cephei.

16, at 8 h. between ϵ and ζ Cephei.

— at 11 h. increased, but not as bright as ζ .

17, at 11 h. rather brighter than ζ .

26, at 11½ h. equal or less than ϵ .

27, at 8 h. evidently brighter than ζ .

Therefore it was between its least and greatest brightness August 31, at noon, and Sept. 26, at 21 h.: these being compared to my first observations, when also between its least and greatest brightness on Nov. 20, at 3 h. and Nov. 30, at 15 h.

1784.

1784, give the following results, the mean of which corroborates that deduced by Mr. GOODRICKE of 5 d. 8 h. 37 $\frac{1}{4}$.

D. H. M.

5 8 35

5 8 41

5 8 33

5 8 39

Length of a single period 5 8 37 on a mean.

3 Cephei concludes the stars of the first class; those that follow are of the second.

HEVELIUS's 6th Cassiopeæ.

In 1782 I first perceived that this star was missing; nor could I find it in 1783 and 1784.

46th or ξ Andromedæ.

This star is said to have diminished in brightness. In 1784 and 1785 I found it, by very exact observations, less than ν , equal to ω^* , and brighter than d and χ ; yet I must mention that it is marked in my journal as being sometimes brighter, and at other times less than ω^* ; but still I am not convinced, that it varies in brightness. FLAMSTEED, in his Catalogue, annexes no character to his 46th Andromedæ; but in vol. II. of his Hist. Cœlest. p. 135. and 138. he marks it ξ .

* I suspect an error in this character, but cannot be certain. H. E.

FLAMSTEED'S 50, 52, τ Andromedæ, and HEVELIUS'S 41 Andromedæ.

As the position and characters of these stars differ considerably in different Catalogues, and that some of them are mentioned by CASSINI to have disappeared and re-appeared, I shall give their brightness as observed in 1783, 1784, and 1785.

FLAMSTEED'S 50th of the 4. 5th magnitude, and equal, if not rather less than ϕ Andromedæ.

———— τ of the 5th magnitude, and equal to 46 and 48 Andromedæ.

————	49	} of the 5. 6th magnitude, and are of the same brightness.
————	52	
HEVELIUS'S	41	

A star between FLAMSTEED'S 52 and HEVELIUS'S 41 is of the 6th magnitude, or rather less. I could not see TYCHO'S 19th Andromedæ; but I take this star to be the same as HEVELIUS'S 41 Andromedæ.

TYCHO'S 20th Ceti.

This must be the star which HEVELIUS said had disappeared, being TYCHO'S second in the Whale's belly. There can hardly be any doubt but that it is the χ , misplaced by TYCHO. This χ is of the 4. 5th magnitude, and of the same brightness as the three ψ Aquarii.

FLAMSTEED'S 55th Andromedæ, marked Neb. in his Catalogue.

It is mentioned in the latest Catalogues of Nebulæ that this nebula could not be found. FLAMSTEED, who, I believe,

only observed it once, *viz.* Oct. 17, 1691, does not mark it nebulous; nor does it appear to me such, but as a star of the 6th magnitude. There are a few small stars near it, which to the naked eye, when the air is very clear, make it appear nebulous, which probably is the reason why FLAMSTEED marked it thus in his Catalogue.

σ or PTOL. and UL. BEIGH's 17th Eridani.

FLAMSTEED says, he could not see this star in 1691 and 1692. In 1782, 1783, and 1784, I observed one of the 7th magnitude in that place; the relative brightness of which appeared always the same, *viz.* less than two little stars near and below γ Eridani.

FLAMSTEED's 41 Tauri.

This star was thought by CASSINI to be a new one or variable. I see little or no reason to be of that opinion; that it is not new is evident, since it is UL. BEIGH's 26th and TYCHO's 43d. In 1784 and 1785 I found it of the 5th magnitude, being equal to ϕ , and brighter than ψ , P, and χ Tauri.

Star about $2^{\circ}4$ North of 53d Eridani, and 47 Eridani.

The first of these stars CASSINI thought a new one, and that it was not visible in 1664. In 1784 I found it was less than u , and d , brighter than A, and seemed equal to ψ Eridani.

CASSINI mentions another star thereabouts, which he also esteemed a new one: this is probably FLAMSTEED's 47th. In 1784 it appeared rather less than 46th.

γ Canis Majoris.

MARALDI could not see this star in 1670; but in 1692 and 1693 it appeared of the fourth magnitude. I have very frequently noticed it since 1782, but perceived not the least variation, being constantly of the 4th magnitude, very little brighter than θ , and decidedly brighter than ι .

 $\alpha \beta$ Geminorum.

If either of these stars have changed in brightness, it is probably the β . In 1783, 1784, 1785, the β was undoubtedly brighter than α .

 ξ Leonis.

MONTANARI says, this star was hardly visible in 1693. I found it constantly in 1783, 1784, and 1785, of the same brightness, being of the 5th magnitude; less than A, π , and, if any difference, rather brighter than b and ω Leonis. TYCHO, FLAMSTEED, MAYER, BRADLEY, &c. mark it of the 4th magnitude.

 ψ Leonis.

This star is said to have disappeared before the year 1667. It is now, and has ever been since 1783, of the 5.6th magnitude, being less than ω , and brighter than i , FLAMSTEED's 46th.

25th Leonis.

In 1783 I first perceived this star was missing; nor was it visible in 1784 and 1785, even with the transit-instrument.

BAYER's ι Leonis, or TYCHO's 16 Leonis.

It was not visible in 1709, nor could I see it in 1785. This is a different star from the ι Leonis of the other Catalogues, though TYCHO's *description* of its place is the same.

 δ Ursæ Majoris.

This star is suspected to change in brightness (see LONG's Astronomy), on account of its being marked by TYCHO, Prince of HESSE, &c. of the 2d magnitude; while HEVELIUS, BRADLEY, and others, have it of the 3d. At present, and for these three years past, it appears as a bright 4th magnitude, being rather less than ι , equal to α , and rather brighter than α Draconis.

 π Virginis.

This star is supposed to be variable, because FLAMSTEED, on the 27th of January, 1680, says he could not see it. He observed it May 12, 1677, and some years afterwards, since it is in his Catalogue. I examined it frequently in 1784 and 1785, without perceiving the least change, being of the 6th magnitude, less than c , and rather brighter than a star three degrees lower in a right line with c and π Virginis.

BAYER'S

BAYER's star of 6th magnitude, 1° South of γ Virginis.

This star is not in any of the nine Catalogues that I have. MARALDI looked for it in vain; and in May, 1785, I could not see the least appearance of it. It certainly was not of the 8th magnitude.

In the northern thigh of Virgo.

This star, which is marked by RICCIOLUS of the 6th magnitude, could not be seen by MARALDI in 1709; nor was it of the 9th magnitude, if at all visible, in 1785.

γ 1 or γ 2 Virginis.

In 1785 I found that one of these stars was missing, and which seems to be the γ 1: the remaining one is of the 6.7th magnitude.

α Draconis.

I am of Mr. HERSCHEL's opinion, that it is highly probable this star is variable. BRADLEY, FLAMSTEED, &c. mark it of the 2d magnitude; at present it is only of a bright 4th. I have frequently examined it since October, 1782, without perceiving the least change, being constantly rather less than α Draconis, equal to δ Ursæ Majoris, and rather brighter than α Draconis.

BAYER's star in the west scales of Libra.

MARALDI says he could not see this star; nor could I in 1784 and 1785. With a night-glass may be seen thereabouts
some

Some small stars of about the 8th magnitude, none of which are near as bright as the 2d $\star$ Libræ.

PROL. and UL. BEIGH's N° 6 of the unformed in Libræ.

In examining different Catalogues I do not find this star in any other than the above, though it is marked of the 4th magnitude. If PROLEMY had not the $\star$ it might be thought to be that. In 1785 I frequently observed a star of the 7th magnitude very near its place, which appeared rather less than FLAMSTEED's 41. FLAMSTEED has not this little star in his Catalogue; but he observed it May 9, 1681.

$\star$ Libræ.

This star is thought to be variable. I am not of that opinion; though certainly it is rather singular that HEVELIUS, whose attention was directed to this part of the heavens, to find TYCHO's 11th, did not observe the $\star$; and the more so, as he has noticed two much lesser stars not far from it. During these three years I have found the $\star$ constantly of the 5th magnitude, being less than ψ or θ , equal to λ , and brighter than γ .

TYCHO's 11th Libræ.

HEVELIUS says he could not find a star of the 4th magnitude in Libræ noticed by TYCHO. This must be TYCHO's 11th, since he has all the others. It was not visible in 1783, 1784, and 1785, nor probably ever existed; for it is, I think, evident, that this 11th is no other than the $\star$, with an error of two degrees in longitude.

33 Serpentis.

In 1784 I perceived that this star was missing; nor was it visible in 1785 with a night-glass.

A star marked by BAYER near ϵ Ursæ Minoris.

CASSINI could not see this star. In 1782 I took, with a night-glass, a plan of all the stars near its place, and near the ϵ , none of which were brighter than the 7.8th magnitude. I have since re-examined the plan, but found no alteration.

The ϵ or Prol. and UL. BEIGH's 14th Ophiuchi or FLAMSTEED's 36th.

I have no doubt but that this is the star which is said to have disappeared before 1695. It is also evident, by what HEVELIUS says in his Catalogue on the θ and B, that the ϵ was not seen by him. In 1784 and 1785 I found it of the 4.5th magnitude, much brighter than 39, also rather brighter than 51 and 58, and less than 44. On the 30th of June, 1783, I have marked it in my journal equal to 39, and less than 51 and 58; but as the observation was not repeated, I am far from being certain it has undergone any change, particularly as this star has a southern declination of 26° , and therefore great attention must be given to the state of the atmosphere.

Prol.

PROL. 13th and 18th Ophiuchi, 4th magnitude.

If there is no error in the Catalogue, these two stars have disappeared; but I am confident that PTOLEMY's 13th is FLAMSTEED's 40th, and that PTOLEMY's 18th ought to be marked with a north latitude instead of south, which would make it agree nearly with FLAMSTEED's 38th.

σ Sagittarii.

Mr. HERSCHEL, with great reason, has placed this star among those which probably have changed their magnitudes. I had long since remarked the singular disagreement in all the Catalogues, which induced me to observe it frequently, particularly in 1783, 1784, and 1785, when it appeared of the 2. 3d magnitude, and brighter than π Sagittarii.

θ Serpentis.

MONTANARI says he saw this star of the 5th magnitude, and that the next year it grew bigger. I examined it frequently in 1783, 1784, and 1785, and found it always less than δ Aquilæ, equal to β Aquilæ, and P Ophiuchi; 4th magnitude.

TYCHO's 27th Capricorni.

This star was not visible in HEVELIUS's time; nor could I see it 1778, 1782, 1784, with the transit-instrument.

TYCHO's 22d Andromedæ and σ Andromedæ.

CASSINI remarked, that the star placed by TYCHO at the end of the chain of Andromeda as of the 4th magnitude, was grown so small that it could scarcely be seen. This is TYCHO's N^o 22, the longitude and latitude of which places it near the two π Cygni, and where no star was visible in 1784 and 1785.

As possibly, by TYCHO's description, CASSINI took the 22d for the σ Andromedæ, I have also examined this star, and in 1783, 1784, and 1785, found its relative brightness thus: less than α Cephei; equal to ζ Cassiopeæ, though, if any difference, rather brighter; and brighter than λ , κ , or ι Andromedæ.

TYCHO's 19th Aquarii.

This is the star that HEVELIUS says was missing, and that FLAMSTEED could not see with his naked eye Nov. 18, 1679; nor could I see the least appearance of it in 1782. I am convinced it is the same star as FLAMSTEED's 56th, marked *f* by BAYER, from which it is only $1^{\circ}\frac{1}{4}$. FLAMSTEED's 53d, marked *f* in PROBLEMY's Catalogue, is a different star.

LA CAILLE's 483 Aquarii.

I first discovered that this star was missing in 1778. It was not visible in 1783, 1784.

There are a few other stars suspected by the ancient astronomers to have been new or altered a little in brightness, which I have omitted, not seeing any reason to think them so; and some that are certainly variable, but cannot be observed in these

these latitudes. I have also, contrary to my first intention, added several which are not mentioned by them; such are those that I lately discovered to be missing.

Perhaps many persons would place in the first class several stars which I have put in the second, relying on the positive assertions we have of their having disappeared, diminished, or being new; for my part, I am confident that most of these supposed changes may be attributed to mistakes; and in general for those that are said to be lost, an attentive comparison of different Maps and Catalogues will point out the error; and thus I have ventured to give my opinion of TYCHO's 20th Ceti, 11th Librae, and 19th Aquarii, &c. Since FLAMSTEED's Catalogue has been more particularly investigated, the number of these supposed lost stars is considerably increased; but if the second volume of his Hist. Cœlest. is also examined, many errors will be detected; among which it will appear very unaccountable, that the 71st, 80th, and 81st of Hercules, which were discovered to be missing by Mr. HERSCHEL, are not in FLAMSTEED's observations under the name of Hercules, though I looked for them with particular attention, and find the 70th. The 19th Persei, which Mr. HERSCHEL also could not see, was observed but once by FLAMSTEED, viz. Jan. 16, 1693, and in all probability is the τ with the time of its transit erroneously set down. The τ was observed on Jan. 17, 1693, and Jan. 18, 1694. Besides these Mr. GOODRICKE has found several other errors still more evident. I scarcely need add, that these corrections do not in the least intimate any mistake or diminish the merit of those that first point them out, but fall entirely on the ancient catalogues and observations.

Mr. HERSCHEL, in selecting several stars which possibly may be reckoned new ones, very judiciously gives us plausible reasons
not

not to lay great stress on their being so; and the following remark only confirms what he there suggests; for that star of the 5th magnitude following τ Persei, mentioned by him, and which with great reason might be esteemed a new one, is in all probability the same as one observed by FLAMSTEED, Jan. 18, 1694, though not inserted in his Catalogue.

With regard to those stars which are said to have diminished or increased, as those in Andromeda, Leo, &c. they are, in my opinion, far from being confirmed as variable. I know, from repeated experience, that even more than a single observation, if not particularised and compared with neighbouring stars, is very little to be depended on; different states of the weather, thin streaks of clouds, have several times made me err a whole magnitude in the brightness of a star.

Whether these apparent changes in the stars proceed intirely from themselves; or whether they are effected by any foreign power that may in part occasion some of their particular appearances and irregularities, we have not sufficient *data* to determine: but whatever are the causes, a division of the different phænomena seems to be the most probable means of forwarding any conjecture that hereafter may be formed; I shall therefore divide the first class into three orders.

The first contains those that are periodical with long intervals; and such I reckon σ Ceti, that in Hydra, that in the Swan's breast and neck, and also MAYER's N° 420.

For the second order I shall mention only three, though others might be added; but the accounts of them are so unsatisfactorily recorded, and their places so little known, that I prefer selecting only that in Cassiopea of 1572, that in Serpentarius of 1604, and that near the Swan's head. The phænomena of these certainly bear a great resemblance to the first:

first: still their sudden appearance, and no certainty of a period, or at least infinitely longer, are, I think, sufficient reasons to separate them.

Lastly, Algol, η Antinoi, β Lyræ, and δ Cephei, are so similar to each other, and so different from the above, that there can be but little or no hesitation in distinguishing them; also the cause of their changes seem in general to be attributed to spots, and a rotation on their axis. This property of the fixed stars, though often suspected, was far from being evident till within these two years; and we are not only indebted to Mr. GOODRICKE for the discovery of the first, but also for three of the only four known.

Further may be added, that all those of the first order (MAYER'S 420 being yet so little known remains doubtful) attain in different periods different degrees of brightness when at their full; also the progressive increase of brightness of that in Hydra, that in the Swan's breast, of α Ceti, β Lyræ, η Antinoi, and δ Cephei, is not similar to their decrease. This peculiarity with regard to Algol is yet uncertain, owing to the rapidness of its changes, so that there is only one that seems to have these points uniform, *viz* the variable in the Swan's neck.

I shall now conclude with the observations from which some results, given in this Paper, have been deduced; they are here collected together, in order to avoid confusion.

Observations

Observations on the variable in Hydra.

n	NORTH	$\downarrow$ of 3d mag.
$\star$ Variable	$\star$	$\downarrow$ of 4th —
x	SOUTH	$\star$ of 6.7th —
		$\star$ of 8th —
		n of 11th —

1783, Dec. 11. A.M. much less than ψ , and rather less than δ .

24. A.M. equal, if not less than, $\downarrow$; of the same colour as $\downarrow$.

1784, Jan. 4. A.M. { brighter than ψ ; of about $\frac{1}{4}$ of the difference between
 ψ and γ ; of a more copper colour than ψ .

9. A.M. } if any difference brighter.

10. A.M. } if any difference brighter.

25. A.M. { with the naked eye, it appeared in brightness nearly between
 ↓ and γ; and of a more copper colour than either ↓ or γ.

Feb. 1. } of the same brightness, but seemed of a more copper
2. } colour.

23. between $\downarrow$ and $\uparrow$; air not clear.

20. In the first part of the text, the author

between ψ and k , but nearer the brightness of ψ .

April 11. 1900.

12-

rather brighter than δ .

have these books in my library. The

c. rather less than k and brighter than x .

9. Father less than x , and brighter than x .

1782, Dec. 1. A.M. did not see the variable; strong moon-light.

Q. A.M. ditto ditto.

22. A.M. dirto dirto.

1785. Mar. 4. or 11. ditto ditto.

April 17. saw the π , but not the variable.

May 4. { visible to the naked eye; less than ψ , and much brighter
than δ : of a more copper colour than δ .

5. } than 4, or a more copper colour than 4,

rather less than $\frac{1}{2}$.

May

- 1785, May 14. equal to $\downarrow$; air not very clear, and moon-light.
 15. equal to $\downarrow$; air clear; moon-light.
 19. undoubtedly brighter than $\downarrow$; little hazy; moon near.
 22. } rather brighter than $\downarrow$; moon-light strong.
 27. }

June 10. rather brighter than $\downarrow$, though I think decreased.

12. equal to $\downarrow$: but am not sure if the sky was quite clear.

13. rather less bright than $\downarrow$; air clear.

Mr. GOODRICKE also frequently observed the variable, and his observations agree with the above.

Observations on γ Antinoi.

h.

- 1785, May 20. at $12\frac{1}{2}$ equal or less than γ Antinoi.
 21. — $12\frac{1}{2}$ equal to γ , evidently less than β ; moon-light.
 22. — $12\frac{1}{2}$ equal if not brighter than β Aquilæ; moon.
 — $12\frac{1}{2}$ thought it brighter than β ; air clear.
 June 10. — $12\frac{1}{2}$ rather brighter than γ Antinoi.
 12. — $11\frac{1}{2}$ rather brighter than γ , less than β .
 13. — $12\frac{1}{2}$ a little brighter than β , much less than γ .
 July 15. — $12\frac{1}{2}$ less than β , brighter than γ .
 17. — 11 less than γ , brighter than μ .
 18. — 11 between γ and β , I think rather nearer β .
 19. — $10\frac{1}{2}$ } between β and γ , rather nearer β ; air clear.
 $11\frac{1}{2}$ }
 20. — 11 rather brighter than β , certainly equal.
 Aug. 30. — $9\frac{1}{2}$ rather brighter than β , at least equal to it.
 31. — 9 much brighter than β .
 Sept. 6. — 9 less than γ ; a single view of it, not very satisfactory.
 7. — $9\frac{1}{2}$ much brighter than β .
 26. — $9\frac{1}{2}$ } much less than β or γ , brighter than μ .
 $10\frac{1}{2}$ }
 27. — 8 } much less than β , less than γ , brighter than μ .
 $10\frac{1}{2}$ }
 28. — $9\frac{1}{2}$ rather brighter than β .
 — 11 increased a little in brightness.

The brightness of the stars to which γ Antinoi is compared are given in the Philosophical Transactions, vol. LXXV. part I.

Observations on the variable in Cygnus's Neck.

	x of 6th mag. d of 7th — e of 7th — a of 8th — b of 8th — c of 8.9 — h of 9th —	As KIRCH's plan (see Phil. Trans. abridged by JONES) is much the same as this. I have annexed the same letters to the stars.
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Dates.	Mag.	
1783		
Mar. 28	0	if visible, not of the 8th magnitude.
June 9	7	rather brighter than d or e .
11	7	rather increased.
23	6.7	less than x .
30	6.7	rather increased.
July 15	6.7	of the same brightness as on the 30th of June.
27	7	a little brighter than d or e .
Aug. 7	7	rather brighter than d ; decreased.
8	7	equal to d .
16	7	less than d , brighter than e .
25	7.8	less than e .
Sept. 4	7.8	think brighter than a .
12	8	equal to a .
22	8.9	much less than a , equal to e , and brighter than b .
24	9	equal to b .
Oct. 14	9.10	less than b .
24	10.11	{ seen with the greatest difficulty; less than any stars of the plan; am doubtful if I saw it by intervals.
27		
1784	0	{ I looked constantly for the variable between October and April, but could not see it.
April 23	10.11	less than any stars of the plan.
24	10.11	ditto.
May 9	9.10	rather less than b .

Dates.	Mag.	
1784		
May 21	8.9	rather brighter than <i>b</i> , but not so bright as <i>b</i> .
June 17	8	equal to <i>a</i> , brighter than <i>b</i> .
July 21	6	equal to χ with the naked eye.
22	6	if any difference the χ was the brightest.
27	6	rather brighter than χ .
Aug. 1	5.6	undoubtedly brighter than χ .
10	6	think it decreased, but still rather brighter than χ .
13	6	rather less than χ .
19		
Sept. 2	6.7	much decreased, less than χ , but brighter than <i>d</i> .
12	6.7	still brighter than <i>d</i> .
19	7	if any difference brighter than <i>d</i> .
20	7	rather less than <i>d</i> .
Oct. 5	7.8	less than <i>e</i> , about equal to <i>a</i> .
10	7.8	rather brighter than <i>a</i> .
Nov. 11	9	less than <i>b</i> , about equal to <i>f</i> .
17	10	rather less than <i>f</i> .
1785		
May 9	0	not visible.
June 21	9.10	less than <i>b</i> .
July 23	8	rather brighter than <i>a</i> .
Aug. 13	7	{ a little brighter than <i>d</i> and <i>e</i> , much less than χ ; am not sure I could see it with the naked eye.
15		
27	6.7	{ not so bright as χ , much brighter than <i>d</i> ; I could see it with the naked eye, but the χ more distinctly.
28	6	{ not quite so bright as χ ; naked eye.
30		
Sept. 2	6	ditto ditto.
7		
11	6.7	{ very difficult to see with the naked eye, decreased in brightness; air clear.
12		
26	7	{ though the air was remarkably clear, it was with the utmost difficulty I could sometimes see it with the naked eye.
27		



X. *An Account of a Subsidence of the Ground near Folkestone; on the Coast of Kent. In a Letter from the Rev. John Lyon, M. A. to Edward King, Esq. F. R. S. and A. S. Communicated by Mr. King in a Letter to Charles Blagden, M. D. Sec. R. S.; with Remarks.*

Read February 16, 1786.

TO DR. BLAGDEN, Sec. R. S.

DEAR SIR,

Mansfield-Street, Dec. 22, 1785.

HAVING always thought the account given in the Philosophical Transactions, by Mr. SACKETTE *, about the beginning of this century, concerning the motion of the Cliffs, and of the adjacent ground, near Folkestone in Kent, a very curious one, and deserving of much attention; both because of the many positive attestations there were of *ancient* men with regard to it (who were both mariners, and used to observation); and because of the singular consequences that would follow, from the ascertaining of such a fact, in a philosophical light; I have constantly, whenever I had any opportunity, made repeated enquiries concerning the matter, of such persons as I thought likely to be able to afford me any satisfactory information.

* See Phil. Transf. vol. XXIX. N^o 349, or JONES's Abridgment, vol. IV. part II. p. 248.

Amongst

Amongst the rest, I mentioned it last summer to my worthy and very curious friend Mr. BOYS, of Sandwich; who seemed surprised at the narration, and had never before heard of any such phænomenon. However, in less than a fortnight after our conversation, I was agreeably surprised by receiving a letter (dated 24th Sept. 1785), in which he said, *I am sorry your health will not permit you to make the tour you at first proposed; especially as something VERY CURIOUS has happened within these few days at Folkestone. Part of the cliff, to the westward of the town, a little way from the church, has sunk, and continues sinking into the earth; raising the ground, about the sinking part, in a very extraordinary manner. This corresponds with what you said to me on the subject of Stufball-Castle, &c.; and certainly deserves your attention. If I could, by going thither, give you any satisfaction, I shall be ready and happy to obey your commands.*

Being, through illness, prevented from examining this curious phænomenon myself, I accepted this obliging offer of Mr. BOYS, and requested his assistance: and although he also was prevented from going to the spot himself, yet he applied to a friend of his, the reverend Mr. LYON, of Dover; who has made repeated visits to the place, to obtain all the information possible; and has, at last, sent to me a very accurate drawing, together with an explanatory letter; which I now, with great pleasure, venture to lay before the Royal Society.

I must, however, at the same time, beg leave to observe, that although Mr. LYON differs from Mr. SACKETTE, in his conclusion, concerning the motion of the whole adjacent country, and controverts *that* fact; and has certainly given a more clear and satisfactory account of the present phænomenon, than Mr. SACKETTE did of that which he wished to record; yet they both agree in imputing a *most remarkable effect* (only in different

different ways) to the passage of the springs, and drains of water, through the stratum of loose marle, on which the whole country rests: an effect which must needs produce, at different periods of time, various alterations on the surface; and may most probably have occasioned much greater changes in the face of the country than that made in the present instance.

Whether therefore Mr. SACKETTE was right or wrong, in his great and final conclusion, concerning the motion of the *whole coast*; what he records, on the testimony of so many aged persons (in which they persisted with great seriousness and on the fullest consideration), does surely still deserve at least to be born in mind, and to be attended to with much circumspection; especially on a coast, where perhaps *fixed points* may be attempted to be ascertained, at some time or other, in order to complete the most accurate and most curious philosophical mensurations.

And I must further venture to observe, in vindication of Mr. SACKETTE's account and conclusions, that although Tarlingham-house has indeed been rebuilt, since the time referred to by the old man who conversed with Mr. LYON; and therefore *that* old man's remark might be occasioned merely by *that* circumstance; yet it had not been rebuilt in Mr. SACKETTE's time; and, therefore, no such circumstance could be the occasion of its coming *recently in view*, at that period when he wrote, in parts of the coast where it had not been possible to see it at some time before.

And as to the MOORING ROCK, *so particularly referred to by Mr. SACKETTE, being now utterly unknown*; it ought to be remembered, that Mr. SACKETTE, in his description of it, says, *that it lies surrounded with great numbers of other rocks, and was on this account chiefly a noted one, because at it vessels use to*

be moored, while they are loading other rocks, which they take from hence, not only for our own pier-heads, but for those of Dover Pier; and a very great quantity of them were shipped in the time of OLIVER's usurpation, and carried to Dunkirk, for the service of that harbour. Considering, therefore, that the enormous pile of Ramsgate Pier has been built since that time, (which, though it be chiefly composed of Portland stone, had, I apprehend, foundations and interior parts of ruder materials) and that there have been other vast demands for stone, it is not at all unlikely, that this very *Mooring Rock*, mentioned by Mr. SACKETTE, has itself been carried away in like manner as the others were that used to surround it; and that this is the sole reason why it is now no longer known, and totally forgotten.

I am, Sir, with much esteem, &c.

EDWARD KING.

P. S. There is a *peculiarity* in Mr. LYON's sketch, Tab. V. (designed to illustrate the grounds of his objections to Mr. SACKETTE's conclusions) which demands some explanation. After having given a section of the cliff and shore, the lines (instead of being continued in the same plane, and in the direction of the *same* section) are drawn so, as to be conceived as extended *on the surface of the country* from the eye of the observer at E. Without attending to this circumstance, what he says is not very easily to be understood; and indeed I must still think, that Mr. SACKETTE does not deserve so much censure, although Mr. LYON's be undoubtedly a most accurate account, and most clear solution of the present phenomenon.

TO

TO EDWARD KING, ESQ.

S I R,

Dover, Nov. 24, 1785.

AS I have been requested by my friend Mr. Boys, of Sandwich, to examine into the cause of the sinking of some ground near the town of Folkestone, in this neighbourhood, and to send you the results of my inquiries; I have made it my business to attend particularly to the subject. I have been twice to view the place. I have endeavoured to procure the best information, and have compared my remarks with what the reverend Mr. SACKETT formerly said upon the same subject to the Royal Society.

That you may have a clearer idea of the place where the ground is sinking, I have annexed a drawing of it, taken from a small hill near the foot of the cliff.

AA (Tab. IV.) represents the length of the ground, 130 feet, which is sunk 40 feet from the top of the cliff DD.

BB, is a fissure, in the valley between the sinking ground AA and the hill HH, and in which there are many smaller chasms.

C, the tower of Folkestone-Church, not far from the cliff DD.

E, part of the town of Folkestone, as seen between the cliff DD and the hill HH.

F, the high chalk cliffs at a distance, leading towards Dover.

G, a track of pasture land, between a high range of hills and the sea.

I, the beach, at the foot of the hill H.

KK, Rocks, said to be raised (and I believe they are) by the sinking of the ground AA.

As I intend, in explaining the cause of the sinking of the ground AA to you, to advance an opinion of my own, and to controvert what the reverend Mr. SACKETT formerly said upon the subject, it may be necessary to explain the nature of the soil, as far as it is open to view, in the neighbourhood of Folkestone.

The chalk cliffs FF, which begin at Dover, form opposite Folkestone town high hills, and leaving the shore, there is a large track of arable and pasture land between them and the sea.

Part of this ground is shewn in the view at G, and is a kind of marle, which contains pyrites, fragments of the Cornu Ammonis, and many other fossil bodies.

Next to the marle is a loose sandy soil (see the cliff DD) intermixed with a very large, hard, and coarse kind of stone, in which are often found fossil oyster shells.

This sandy soil rests upon a marle, which at the cliff DD is in some places three or four feet above the beach, and when wet is very slippery. A stratum of this marle extends for many miles on the coast, and where it is not sufficiently covered with sand to bear any weight, it is in many places a quag, and dangerous to pass over.

Through this track of land I have described, there are many drains of water, which may be supplied partly from the falling of the rains in wet seasons, and partly from the springs issuing from the hills; and there is reason to suppose, that in a loose soil these drains form channels in a course of time. At the place where the ground has sunk before, and is now sinking, there is a drain from the marle under the sand; and I am of opinion, that the course of the water is in the same direction.

as the valley between the hill H and the sinking of the ground AA.

That the sinking of the ground is caused by the foundation being undermined (and I think by water) is evident from the appearance of the ground in the valley. The soil is full of fissures, and resembles an arch, which is sunk down, and has left the two abutments, the hill H and the cliff DD, standing.

As the hill H more than counter-balances the pressure of the sinking ground upon the stratum of wet marle, the consequence is, that the rocks KK, at some yards distance, being only thinly covered with sand, are forced upwards, and become visible, and the wet marle in many places is squeezed through the sand with them.

(This appears to me to be the true reason of the sinking of the ground at one place, and the rising of the rocks at another.

That the reverend Mr. SACKETT's account of the sinking of the ground at Folkstone, to the Royal Society, is founded in error, I have not the least doubt, from the present appearance of some of the objects he describes. I am rather at a loss to follow him exactly, as the oldest man in the town of Folkstone (I am told) never heard of the Mooring-rock he mentions.

I think by his description the sinking of the ground must have been in his time at the same place it is now, as Tarlingham-house is not to be seen on the other side of the town.

Admitting this to be the case, there will still be a difficulty respecting the relative situation of each place in explaining what he calls a sketch of the country. But, to explain my meaning more fully, let B (Tab. V.) represent the
foot

foot of the hill H in the view, which is upwards of 30 feet high.

CD, the valley between the hill B and the cliff.

DE, the craggy cliff, 60 yards high.

EF, a plain, above a mile long.

FG, a hill of steep ascent, Mr. SACKETTE says near half a mile; but this is much higher than it really is.

GH, the land from the top of the hill to the house near a mile.

I, Tarlingham-house, lying two miles and a half N.N.W. from the rock.

EGH, a line of sight (see Mr. SACKETTE's description of the country).

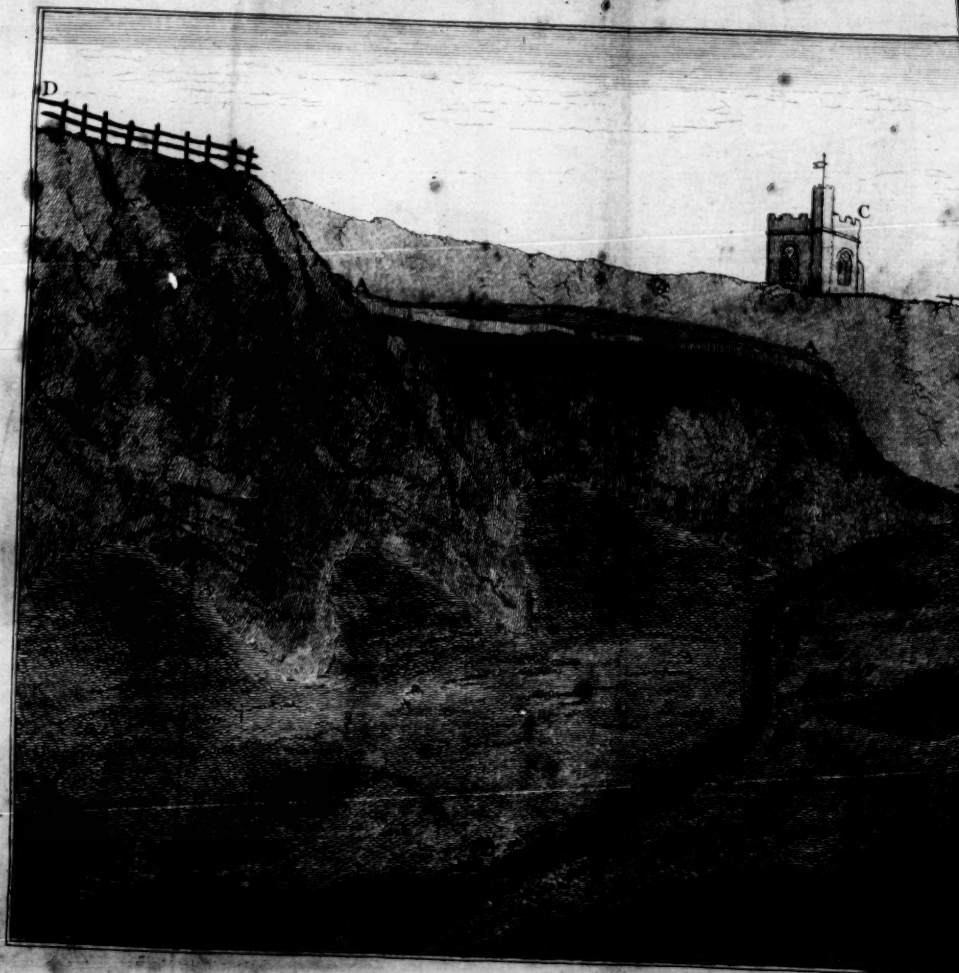
If Mr. SACKETTE, in the above description of his sketch of the country, had placed each object according to its real situation; and if the effects he has mentioned had been real ones, they would have been truly wonderful, and worthy the attention of the curious investigator of the hidden operations of nature; but I am apprehensive he had but very little better foundation for what he has said than the vague and inconsistent reports of a few ancient fishermen. Tarlingham-house is by Mr. SACKETTE's account situated full as far beyond the hill FG as the width of the plain EF; but how deep the hill has sunk to render the house visible over the top must depend upon the situation of it, *viz.* how much higher it was than the top of the hill.

If the hill has sunk only ten feet, there must have been some external evidence of it, such as fissures round the base, and a very steep ascent from the top of it, where the separation happened between it and Tarlingham-house; but there are no traces of any such sinking of the hills.

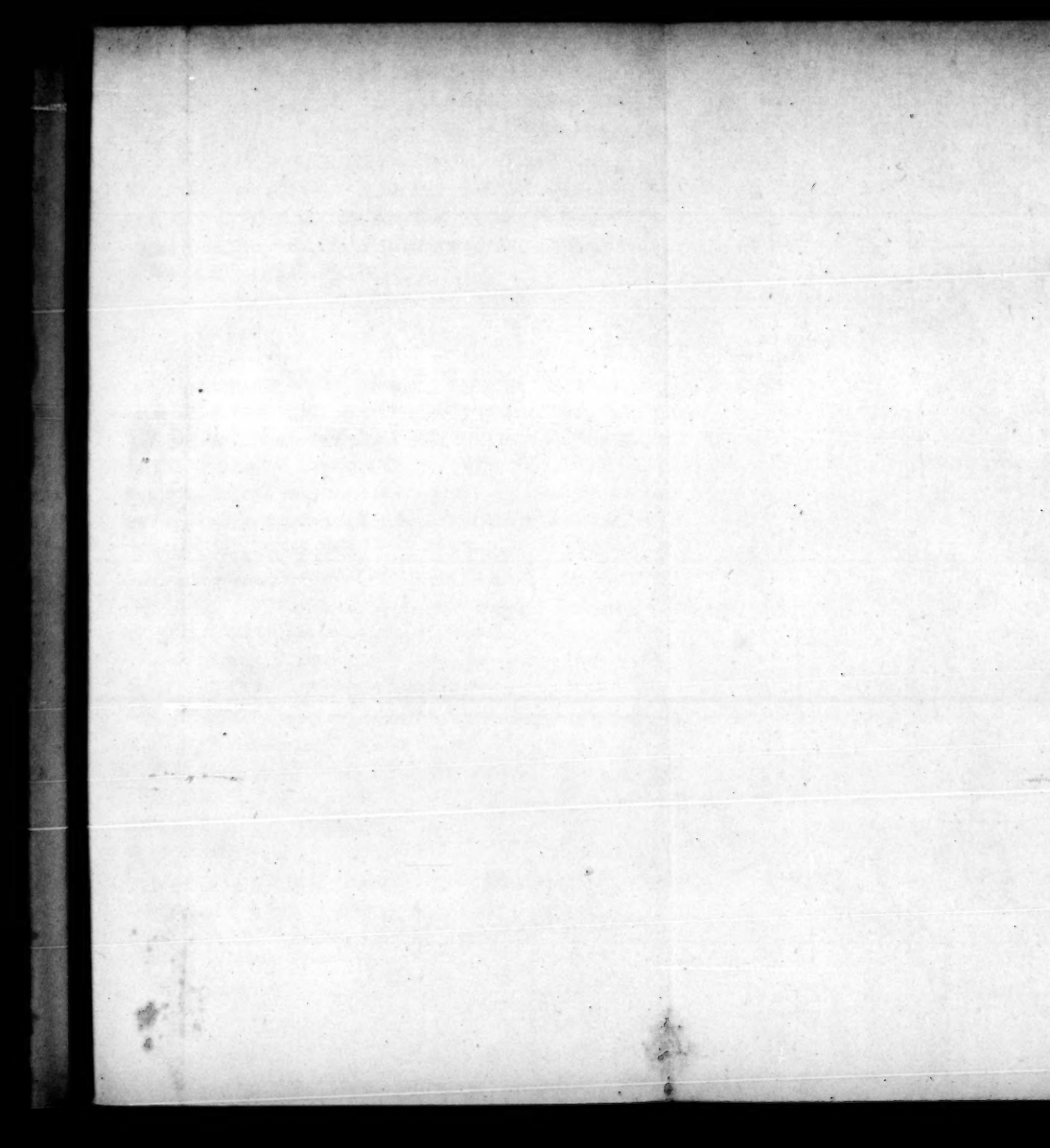
There is farther proof that Mr. SACKETTE did not examine into the matter himself, but rested what he said upon the report of others; and this is, that Tarlingham-house is not seen over the top of the hill in the line of sight EG, but considerably to the left of it, in the line EI, and clear even of the base of the hill. Besides, a moment's reflection would have told him, that the sinking of the hills could not produce the effects he mentions; for if the ground in the plain was pushed forward by it, it could not be a partial slipping; not only the church, and the whole town, must have been removed, but every object between the base of the hills and the cliff must have been removed out of their place; but I may venture to affirm, there is no proof of this having been done. I should have been drawn into the same or similar errors myself, if I had rested satisfied with the first accounts I received from an ancient fisherman. He told me the same story of the hills sinking in his time, and Tarlingham-house appearing higher than it did since he could remember. In one part of his relation he was right; for I found, upon inquiry, that Tarlingham-house has been taken down, and built upon a much larger scale than formerly, since it has been in the hands of the present proprietor.

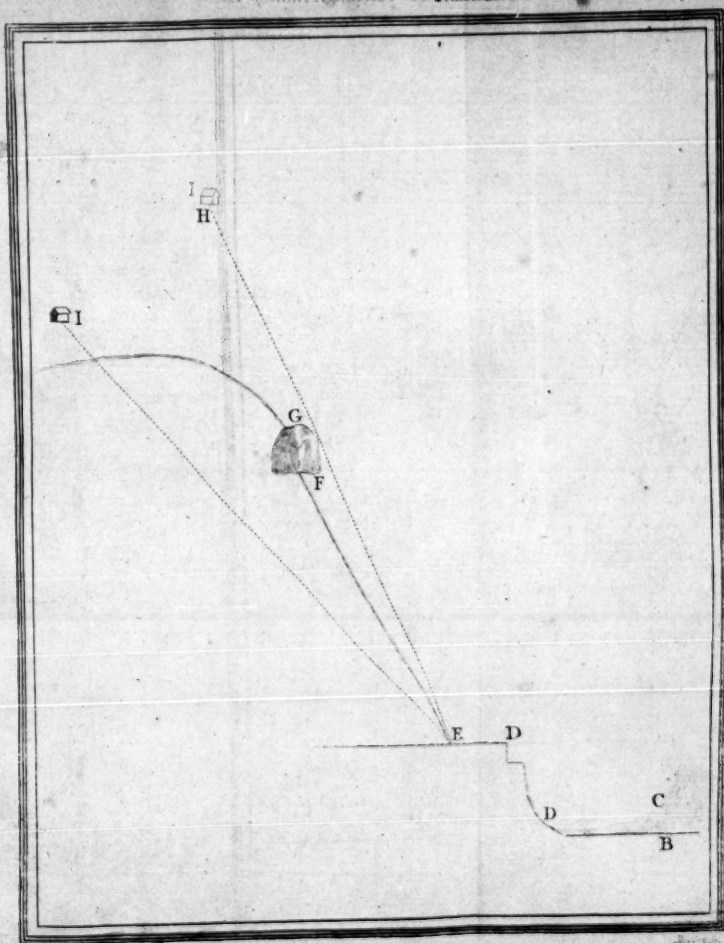
If what I have said should not prove satisfactory, I shall be happy in giving you any farther information upon this subject in my power; and am, Sir, &c.

J. LYON.









XI. Particulars relative to the Nature and Customs of the Indians of North-America. By Mr. Richard M^c Caulsland, Surgeon to the King's or Eighth Regiment of Foot. Communicated by Joseph Planta, Esq. Sec. R. S.

Read February 16, 1786.

IT has been advanced by several travellers and historians that the Indians of America differed from other males of the human species in the want of one very characteristic mark of the sex, to wit, that of a beard. From this general observation, the Esquimaux have been excepted; and hence it has been supposed, that they had an origin different from that of the other natives of America. Inferences have also been drawn, not only with respect to the origin, but even relative to the conformation of Indians, as if this was in its nature more imperfect than that of the rest of mankind.

It appears somewhat singular that authors, in deducing the origin both of the Esquimaux and of the other Indians of America from the old world, should never have explained to us how the former came to retain their beards, and the latter to lay them aside. To ascertain the authenticity of this point may perhaps prove of little real utility to mankind; but the singularity of the fact certainly claims the attention of the curious; and as it is impossible to fix any limits to the in-

ferences

rences which may at one time or another be drawn from alleged facts, it must always be of consequence to inquire into the authenticity of those facts, how little interesting soever they may at present appear.

I will not by any means take upon me to say that there are not nations of America destitute of beards; but ten years residence at Niagara, in the midst of the Six-Nations (with frequent opportunities of seeing other nations of Indians) has convinced me, that *they* do not differ from the rest of men, in this particular, more than one European differs from another: and as this imperfection has been attributed to the Indians of North-America, equally with those of the rest of the Continent, I am much inclined to think, that this assertion is as void of foundation in one region as it is in the other.

All the Indians of North-America (except a very small number, who, from living among white people, have adopted their customs) pluck out the hairs of the beard; and as they begin this from its first appearance, it must naturally be supposed, that to a superficial observer their faces will seem smooth and beardless. As further proof that they have beards, we may observe, first, that they all have an instrument for the purpose of plucking them out. Secondly, that when they neglect this for any time, several hairs sprout up, and are seen upon the chin and face. Thirdly, that many Indians allow tufts of hair to grow upon their chins or upper lips, resembling those we see in different nations of the old world. Fourthly, that several of the Mohocks, Delawares, and others, who live amongst white people, sometimes shave with razors, and sometimes pluck their beards out. These are facts which are notorious amongst the Army, Indian-Traders, &c.; and which

are never doubted in that part of the world by any person in the least conversant with Indians: but as it is difficult to transport a matter of belief from one country to another distant one, and as the authors who have maintained the contrary opinion are too respectable to be doubted upon light grounds, I by no means intend to rest the proofs upon what has been said, or upon my single assertion.

I have provided myself with two authorities, which I apprehend may in this case be decisive. One is Colonel BUTLER, Deputy Superintendent of Indian Affairs, well known in the late American war, whose great and extensive influence amongst the Six-Nations could not have been acquired by any thing less than his long and intimate knowledge of them and their language. The other authority is that of THAYENDANEGA, commonly known by the name of Captain JOSEPH BRANT, a Mohock Indian of great influence, and much spoken of in the late war. He was in England in 1775, and writes and speaks the English language with tolerable accuracy. I shall therefore only subjoin their opinions upon this matter, the originals of which I have under their own signatures.

Colonel BUTLER's.

THE men of the Six-Nation Indians have all beards naturally, as have all the other nations of North-America which I have had an opportunity of seeing. Several of the Mohocks shave with razors, as do likewise many of the Panees who are kept as slaves by the Europeans. But in general the Indians pluck out the beard by the roots from its earliest appearance; and as their faces are therefore smooth, it has been supposed that they were destitute of beards. I am even of opinion, that
if

if the Indians were to practise shaving from their youth; many of them would have as strong beards as Europeans.

(Signed)

Niagara, April 12, 1784.

JOHN BUTLER.

Agent of Indian Affairs.

Captain BRANT's.

THE men of the Six-Nations have all beards by nature; as have likewise all other Indian nations of North America which I have seen. Some Indians allow a part of the beard upon the chin and upper lip to grow, and a few of the Mohocks shave with razors in the same manner as Europeans; but the generality pluck out the hairs of the beard by the roots as soon as they begin to appear; and as they continue this practice all their lives, they appear to have no beard, or at most only a few straggling hairs which they have neglected to pluck out. I am however of opinion, that if the Indians were to shave they would never have beards altogether so thick as the Europeans; and there are some to be met with who have actually very little beard.

(Signed)

JOS. BRANT THAYENDANEGA.

Niagara, April 19, 1783.

Upon this subject I shall only further observe, that it has been supposed by some, that this appearance of beard on Indians arises only from a mixture of European blood; and that an Indian of pure race is intirely destitute of it. But the nations, amongst whom this circumstance can have any influence, bear so small a proportion to the multitude who are unaffected
by

by it, that it cannot by any means be considered as the cause; nor is it looked upon as such either by Captain BRANT or Colonel BUTLER.

I shall here subjoin a few particulars relative to the Indians of the Six-Nations, which, as they seem not to be well understood even in America, are probably still less known in Europe. My authorities upon this subject, as well as upon the former, are the Indian Captain BRANT and Colonel BUTLER.

Each nation is divided into three or more tribes; the principal of which are called the Turtle-tribe, the Wolf-tribe, and the Bear-tribe.

Each tribe has two, three, or more chiefs, called Sachems; and this distinction is always hereditary in the family, but descends along the female line: for instance, if a chief dies, one of his sister's sons, or one of his own brothers, will be appointed to succeed him. Among these no preference is given to proximity or primogeniture; but the Sachem, during his life-time, pitches upon one whom he supposes to have more abilities than the rest; and in this choice he frequently, though not always, consults the principal men of the tribe. If the successor happens to be a child, the offices of the post are performed by some of his friends until he is of sufficient age to act himself.

Each of these posts of Sachem has a name which is peculiar to it, and which never changes, as it is always adopted by the successor; nor does the order of precedency of each of these names or titles ever vary. Nevertheless, any Sachem, by abilities and activity, may acquire greater power and influence in the nation

than those who rank before him in point of precedency; but this is merely temporary, and dies with him.

Each tribe has one or two chief warriors; which dignity is also hereditary, and has a peculiar name attached to it.

These are the only titles of distinction which are fixed and permanent in the nation; for although any Indian may by superior talents, either as a counsellor or as a warrior, acquire influence in the nation, yet it is not in his power to transmit this to his family.

The Indians have also their *Great Women* as well as their *Great Men*, to whose opinions they pay great deference; and this distinction is also hereditary in families. They do not sit in council with the Sachems, but have separate ones of their own.

When war is declared, the Sachems and great Women generally give up the management of public affairs into the hands of the warriors. It may however so happen, that a Sachem may at the same time be also a chief warrior.

Friendships seem to have been instituted with a view towards strengthening the union between the several nations of the confederacy; and hence *Friends* are called the sinews of the Six-Nations. An Indian has therefore generally one or more *friends* in each nation. Besides the attachment which subsists during the life-time of the two friends, whenever one of them happens to be killed, it is incumbent on the survivor to replace him, by presenting to his family either a scalp, a prisoner, or a belt consisting of some thousands of wampum; and this ceremony is performed by every *friend* of the deceased.

The purpose and foundation of war parties therefore, is in general, to procure a prisoner or scalp to replace the friend or relation of the Indian who is the head of the party. An Indian

dian who wishes to replace a friend or relation presents a belt to his acquaintance, and as many as chuse to follow him accept this belt, and become his party. After this, it is of no consequence whether he goes on the expedition or remains at home (as it often happens that he is a child), he is still considered as the head of the party. The belt he presented to his party is returned fixed to the scalp or prisoner, and passes along with them to the friends of the person he replaces. Hence it happens, that a war party, returning with more scalps or prisoners than the original intention of the party required, will often give one of these supernumerary scalps or prisoners to another war party whom they meet going out; upon which this party, having fulfilled the purpose of their expedition, will sometimes return without going to war.



shed a steady shower to basin a copious of soft rain and
 29.000 mil. water of 2000 as rain as has continued to 20

**XII. Abstract of a Register of the Barometer, Thermometer, and
 Rain at Lyndon in Rutland, in 1785. By Thomas Barker,
 Esq. Also of the Rain at South Lambeth, in Surrey; and at
 Selbourn and Ryfield, Hampshire. Communicated by Thomas
 White, Esq. F.R.S.**

Read February 23. 1786.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.			Lyndon.	S. Lambeth.	Selbourn.	Ryfield.
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,83	28,59	29,31	45½	34½	39	45	25½	35				
	Aftern.				46	34½	40	47½	27	38½	1,494	1,78	2,84	2,12½
Feb.	Morn.	30,16	28,45	29,34	39	29	34½	38	10	28				
	Aftern.				40	30½	36	42½	23	34	0,365	1,20	1,80	1,85
Mar.	Morn.	29,84	29,16	29,61	44	29½	36	43½	17	30½				
	Aftern.				45	31½	37½	51	27½	38	0,212	0,35	0,30	0,00½
Apr.	Morn.	30,05	28,88	29,71	50	36	48	52½	25	41				
	Aftern.				58	37	50	67½	37½	54	0,175	0,34	0,17	0,14½
May	Morn.	30,09	28,95	29,54	61	50½	55	68½	42	48				
	Aftern.				64	51½	56½	75½	50½	60½	0,666	0,81	0,60	0,96
June	Morn.	29,99	29,32	29,71	66½	53	61	60	48	55				
	Aftern.				69½	55½	63	80½	54½	69	1,567	2,04	1,39	1,19
July	Morn.	29,82	28,97	29,42	68½	60	64	64	53½	58½				
	Aftern.				73	61½	65	83	60½	70	3,283	1,73	3,80	1,69
Aug.	Morn.	29,72	28,99	29,36	64½	55½	59½	59½	43½	53½				
	Aftern.				65	56½	61	71	58	64	4,315	3,05	3,21	4,26
Sept.	Morn.	29,89	28,51	29,29	63	60	59	59	36	52				
	Aftern.				64½	51½	60	72	47	63	3,314	2,75	5,94	5,30
Oct.	Morn.	29,99	28,95	29,45	58	41½	50½	55½	28	42½				
	Aftern.				59½	43	51½	62½	37½	52	1,653		5,21	2,52
Nov.	Morn.	30,02	28,33	29,33	51½	39	44½	49½	28	37				
	Aftern.				52½	39½	45	56½	34½	44	1,125		2,27	1,46½
Dec.	Morn.	29,79	28,76	29,32	43½	32	39	43	20½	33				
	Aftern.				44½	32½	40	45½	24	37	2,037	1,53	4,02	3,04
											Inches 20,206	19,62	31,55	24,55½

The severe frost of December, 1784, broke early in January, and was all gone before the middle, and the most open part of this sharp winter followed it, being misty or thick and warm very wet air; but the last day of January another frost set in, which, though not so steady as the former, was sometimes very severe, and did not go away till near the middle of March; and this winter, particularly the former frost about December 10, was much severer in the south of England than here, and greater signs of destruction by it were seen among the trees and plants there. From the breaking of the frost till April 4, was chiefly frosty mornings, and sometimes in the shade all day, so that, if you count the number of frosty days, I do not know that any winter had more, though I have known several longer frosts, and more steady, and some few more severe.

From April 5, the weather began to mend, was tolerably pleasant, and things came on gradually; yet not without some frosty mornings, even in May. The seed time began late, but was without hindrance; and there having been very little rain since the frost, it harrowed remarkably fine, and the lands and roads were uncommonly dusty. The corn came up very well, except the late sown, some of which, especially in the south of England, lay dry till June; for it continued a remarkably dry time all spring, so that the grass was very short, and hay very scarce; yet the grain continued particularly fine-coloured, and eared very well, though some of the winter corn was rather thin; yet that was much mended by some refreshing showers in May and June, which were enough to freshen things, though not to make much grass: and during this drought there were great numbers of little whirlwinds, sometimes several in a day.

The

The weather began to be showery the middle of July, and several great rains; and after August 3d it was more frequent, but less at a time. This made plenty of good grass, but was very troublesome for the harvest, which was got in slowly, and with loss, but came out again full as well as could be expected. The wheat was remarkably full-eared. The barley good, except the late sown, which never ripened; and some too hastily carried in harvest. The birds of passage went away rather early this year: almost all the Swifts were gone in July, and most of the Swallows and Martins in September; the last were August 7, and October 12. It continued very showery till near the middle of October; after which the autumn was pretty fine, and less wet than before, yet enough to make it very dirty when the sun lost its power in December; and the winter began for the most part open and pleasant, till a frost and large snow at Christmas, which grew severer to the end of the year.

On the Variations of Seasons.

Measuring the rain for a few years will not shew completely the general quantity of rain which falls in any place; for there is a very great difference at different periods of time. If I had measured the rain at Lyndon only in the four years 1740, 1741, 1742, 1743, the mean would have been found to be only 16½ inches in a year; yet they were not all complained of as dry summers. 1740 was cold and dry till July 30. The spring

spring 1741 was cold and dry, the summer hot, dry, and burning till the beginning of September; then ten days wet and very warm again, being the finest autumn for grass ever known. 1742 was a showery summer, and 1743 wet in the middle; but then the winters were dry, so that the quantity of rain upon the whole was small. 1741 to 1750 the mean was 18½ inches. 1741 and 1750 were hot, dry, and burning, 1750 being the hottest year I have known. The intermediate years were neither very wet nor very dry; and this was the most plentiful and cheapest time for corn of any ten years I remember; for grain oftener fails in England from too much wet than too little. 1751 to 1760 the mean year was 22¼. 1760 was hot, dry, and burning; but several of the summers were wet, and the crops not so plentiful. Three wet summers together, 1754, 1755, and 1756, were a time of scarcity, and we have had more failing crops since that time than before it. From 1761 to 1770 there was 23½ in a year. 1762 was hot, dry, and burning; and 1765 cold and dry; but several years were wet, 1763 and 1768 remarkably so; and of those ten years several had failing crops, and some had great snows. There was a great change of the seasons at 1763; for I have had more rain since that time than I had before it in the proportion of 5 to 4. From 1770 to 1780 there was at a mean 26 inches. 1771 was dry, and 1778 and 1779 were hot, yet not without fits of rain; and most of the other years were wet, and some great snows. 1773, 1774, and 1775, were so wet that there came 32 inches in a year, which is nearly double what there was from 1740 to 1743. In twelve months, from October 1773, to September 1774, there came 39.390 inches of rain, which is nearly a Lancashire year. And in

one month, September 1774, there was 8 inches: this was in barley and pease harvest, and for three weeks together not a load could be carried in. By the above state of the case it appears, that, for four successive periods of ten years, the quantity of rain has been increasing each time.



[24th] 1786
XIII. *An Account of Experiments made by Mr. John M^r Naby,
at Henley House, Hudson's Bay, relating to freezing Mix-
tures. By Henry Cavendish, Esq. F. R. S. and A. S.*

Read February 23, 1786.

IN my observations on Mr. HUTCHINS's Experiments, printed in the LXXIII^d volume of the Philosophical Transactions, I gave my opinion concerning the cause of the cold produced by mixing snow with different liquors. As there were some circumstances, however, which seemed to form a difficulty in the way of this opinion, I was desirous of having further experiments made on the subject; and at the same time I thought that, by proper management, a greater degree of cold might be produced than had hitherto been done. On mentioning the experiments I wished to have made to Mr. HUTCHINS, he very obligingly desired Mr. M^r NAB, Master at Henley-House, to try them; who was so good as to undertake the business, and has executed it in the most satisfactory manner; as he has not only taken great pains, but has shewn the utmost attention and accuracy, in observing and relating all the phenomena which occurred, and has manifested great judgement in frequently adapting the manner of trying the experiments to appearances which occurred in former ones, to which we are indebted for great part of the most curious facts in this paper. His endeavours have also been attended with much success, as he has not only shewn many remarkable circumstances relating to the freezing of the nitrous

and vitriolic acids, and the phenomena of freezing mixtures; but has also produced degrees of cold greatly superior to any before known.

1. In the above-mentioned Paper I said, that the cold produced by mixing spirit of nitre with snow, is owing to the melting of the snow; and that in all probability there is a certain degree of cold, in which spirit of nitre is so far from dissolving snow, that it will yield out part of its own water, and suffer that to freeze, as is the case with solutions of common salt; so that if the cold of the materials, before mixing, is equal to this, no additional cold can be produced. A circumstance, however, which at first sight seems repugnant to this opinion, occurred in an experiment of FAHRENHEIT's for producing cold by a mixture of spirit of nitre and ice; namely, that the acid, which had been repeatedly cooled by different frigorific mixtures, was found frozen before it was mixed with the ice; notwithstanding which, cold was produced by the mixture. Professor BRAUN also found, that cold was produced by mixing frozen spirit of nitre with snow. On consideration, however, this appeared by no means inconsistent with the opinion there laid down, as there was great reason to think, that the freezing of the acid was of a different kind from that considered in the above-mentioned Paper, and that it did not proceed from the watery part separating from the rest and freezing; but that the whole acid, or perhaps the more concentrated part, froze; in which case it would not be extraordinary that the acid should dissolve more snow, and produce cold.

2. To clear up this point, I sent to Hudson's Bay a bottle of spirit of nitre, of nearly the same strength as FAHRENHEIT's; and desired Mr. M^r NAB to expose it to the cold, and, if it froze, to ascertain the temperature, and decant the fluid part into
3
another

another bottle, and send both home to be examined, as it would thereby be known, whether it was the whole acid, or only the watery part, which froze. For the same purpose also I sent some dephlogisticated spirit of nitre of the same strength, and also some strong oil of vitriol. I also sent some spirit of nitre and spirit of wine, both diluted with so much water, that it was expected, that with the cold of Hudson's Bay they would suffer the first kind of congelation; that is, their watery part would freeze, and thereby make the difference between the two kinds of freezing more apparent.

3. In the same Paper I say, "That on adding snow gradually to some of the spirit of nitre used by Mr. HUTCHINS, I found, that the addition of a small quantity produced heat instead of cold; and it was not until so much was added as to increase the heat from 28° to 51° , that the addition of more snow began to produce cold; the quantity of snow required for this purpose being pretty exactly one quarter of the weight of the spirit of nitre, and the heat of the snow and air of the room, as well as the acid, being 28° . The reason of this is, that a great deal of heat is produced by mixing water with spirit of nitre, and the stronger the spirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted snow was greater than the cold produced by the melting of the snow; and it was not till it was diluted by the addition of one quarter of its weight of that substance, that the cold generated by the latter cause began to exceed the heat generated by the former. From what has been said, it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite so great as that made with

"the same acid, diluted with a quarter of its weight of water;
 "supposing the acid and snow to be both at 28° of heat; and
 "there is no reason to think, that the event will be different if
 "they are colder; for the undiluted acid will not begin to
 "generate cold, until so much snow is dissolved as to increase
 "its heat from 28° to 51° , so that no greater cold will be
 "produced, than would be obtained by mixing the diluted acid
 "heated to 51° with snow of the heat of 28° . This method
 "of adding snow gradually to an acid, is much the best way
 "I know of finding what strength it ought to be of, in order
 "to produce the greatest effect possible."

As it seemed likely that, by following this method, a greater
 degree of cold might be produced than had been done hitherto,
 I sent three other bottles of spirit of nitre and oil of vitriol, all
 three diluted, but not so much so, but that I thought they
 would require a little farther dilution, in order to reduce them
 to their properest degree of strength. I also sent a bottle of
 highly rectified spirit of wine, and a mixture of equal quanti-
 ties of the above-mentioned common spirit of nitre and oil of
 vitriol; and desired Mr. M^r NAB to find what degree of cold
 could be produced by mixing them with snow, after having
 first reduced them, in the above-mentioned manner, to their best
 degree of strength*.

He was also desired to ascertain how much snow he added;
 for as their strength was determined before they were sent out,
 it would thereby be known what was the best strength of these
 liquors for frigorific mixtures.

* This might have been done at home; but I thought it not unlikely that the
 strength found this way might differ, in some measure, according to the heat in
 which the experiment was tried.

All these bottles were numbered with a diamond; and as I shall sometimes distinguish them by these numbers, and as it may be of use to those who may consult the original, I have added the following list of these bottles, with their contents.

N ^o .	Liquors mentioned in Art. 3.	Weight of marble which they dissolve.	Specific gravity at 60° of heat.
168	Spirit of nitre, ...	.582	1.4371
27	Dephlogisticated spirit of nitre, ...	.53	1.4040
103	Diluted oil of vitriol, ...	.654	1.5596
28	Equal weights of N ^o 168. and N ^o 103.	— —	— —
8	Very highly rectified spirit of wine, ...	— —	.8195
Liquors mentioned in Art. 2.			
151	Strong oil of vitriol, ...	.98	1.8437
142	Spirit of nitre, ...	.525	1.4043
139	Some of the same diluted with twice its weight of water, ...	— —	— —
141	Dephlogisticated spirit of nitre, ...	.53	1.4033
143	Some of the same spirit of wine as in N ^o 8. } diluted with 1½ its weight of water, ... }	— —	— —
72	Diluted oil of vitriol for comparing the thermometers, ...	.629	— —
171	Oil of vitriol of about the usual strength, but the exact strength not known, intended to refresh the former when too weak.	— —	— —

4. Professor BRAUN says, that by mixtures of snow and spirit of nitre he sunk thermometers filled with oil of Cassiafras, and some other essential oils, to -100° or -124° ; and that, by the same means, he sunk thermometers filled with the highest rectified spirit of wine to -148° . Though there seemed great reason to think, from Mr. HUTCHINS's experiments, that there must be some mistake in this; yet, as it was possible that the essential oils, and even spirit of wine of a strength much different from that with which Mr. HUTCHINS's thermometers were filled, might follow a considerably different progression in their contraction

contraction by great degrees of cold, I sent a thermometer filled with oil of saffraſas, and two others with ſpirits of wine. One of theſe laſt was filled with the higheſt rectified ſpirits I could procure, its ſpecific gravity at 60° of heat being ,8185; the other was intended to be filled with common ſpirits, though from circumſtances I am inclined to ſuſpect *that* alſo to have been filled with the beſt ſpirits. Beſides theſe, there was ſent a mercurial thermometer, accurately adjusted, according to the directions of the Committee of the Royal Society, printed in the LXVIIth volume of the Transactions; and alſo the two ſpirit thermometers uſed by Mr. HUTCHINS, which were filled with ſpirits whoſe ſpecific gravity was ,8247.

5. Theſe thermometers were compared together by expoſing them to the cold, with their balls immerſed in a glaſs veſſel filled with diluted oil of vitriol. They were at times alſo compared in cold more violent than the natural cold of the climate, by adding ſnow to the acid in which they were tried, in which caſe care was taken to keep the mixture frequently ſtirred. Oil of vitriol was recommended for this purpoſe, as a fluid which would moſt likely bear any degree of cold without freezing, and whoſe natural cold might be much increaſed by the addition of ſnow. It ſeems to have answered the purpoſe very well, and not to have been attended with any inconvenience.

During the firſt compariſon of theſe thermometers, a whitish globule, ſuch as thoſe which appear in frozen oil, was obſerved in the tube of the thermometer filled with oil of ſaffraſas. This appearance of congelation did not much increaſe; but two days after a large air bubble was found in its ball, which prevented Mr. Mc NAB from making further obſervations with it.

It

It is well known, that spirit of wine expands more by a given number of degrees of a mercurial thermometer in warm temperatures than in cold ones; and this inequality, as might be expected, was less in the stronger spirit than in the weaker, but the difference was inconsiderable. The oil of saffrafas also had some of this inequality, but much less. It however appears to be by no means a proper fluid for filling thermometers with. No appearance was observed which indicates any considerable irregularity in the contraction of spirits of wine in intense cold, or which renders it probable, that thermometers filled therewith could be sunk by a mixture of snow and spirit of nitre to a degree near approaching to that mentioned by Professor BRAUN.

6. Mr. M^c NAB in his experiments sometimes used one thermometer and sometimes another; but in the following pages I have reduced all the observations to the same standard; namely, in degrees of cold less than that of freezing mercury I have set down that degree which would have been shewn by the mercurial thermometer in the same circumstances; but as that could not have been done in greater degrees of cold, as the mercurial thermometer then becomes of no use, I found how much lower the mercurial thermometer stood at its freezing point, than each of the spirit thermometers, and increased the cold shewn by the latter by that difference.

On the common and dephlogisticated Acids of Nitre.

The following experiments shew, that both these acids are capable of a kind of congelation, in which the whole, and not merely the watery part, freezes. Their freezing point also differs

differs greatly according to the strength, and varies according to a very unexpected law. Like water too they bear being cooled very much below their freezing point before the congelation begins, and as soon as that takes place, immediately rise up to the freezing point.

7. On the morning of Feb. 1. the common and dephlogisticated spirits of nitre, N° 142 and 141, whose specific gravities were 1,4043 and 1,4033, were found clear and fluid, the cold of the air at that time being -47° . They also bore being shook without any alteration; but on taking out their stoppers, both of them in a few minutes began to freeze, the congelation beginning by a white appearance at top, which gradually spread to the bottom; and they became so thick as not to move on inclining the phial. For want of a thermometer whose ball reached far enough below its scale, Mr. Mc NAB was not able to determine their cold while in the bottle; but in somewhat more than an hour's time, the frozen acid had so much subsided as to admit of his pouring a little fluid matter out of each into a glass with a thermometer in it*; whereby the cold of the common spirit of nitre was found to be $-31^{\circ}\frac{1}{2}$, and that of the dephlogisticated acid -30° , the temperature of the air being -41° . Each of these decanted liquors, at the time their temperature was tried, was full of small *spicula* of ice: they were then put into phials well stopped, and they, as well as the undecanted liquors, sent home to be examined. The decanted part of the common

* It may be asked, why it was more possible to decant any liquor at this time than at first, as the acid was all the while exposed to a cold much below the freezing point? The reason in all probability is, not that any part of the ice first formed dissolved, but that the small filaments into which it first collected together, and in some measure subsided to the bottom,

Spirit of nitre dissolved, 535, of its weight of marble, and the undecanted part, 523; for which reason I shall call the strength of the former, 535, and that of the latter, 523; which mode of reckoning is observed in the remainder of this Paper. The strength of the decanted part of the dephlogisticated acid was 556, and that of the undecanted part, 528; so that it appears that in each of these acids the unfrozen part was a little stronger than the frozen part. It is remarkable, that in the common spirit of nitre, the decanted part, though stronger than the other, was paler coloured and less fuming.

8. On Dec. 21, the temperature of the air being -28° , some dephlogisticated spirit of nitre (N^o 27.) of nearly the same strength as the former acid, was poured into a jar, in order to be diluted with snow, as recommended in Art. 2. Immediately after it was decanted, it began to freeze, in the same manner as before described, except that a less portion of it seems to have congealed: its temperature, tried by dipping a thermometer into it, was -19° , where it remained stationary for many minutes; it was then diluted with snow, as will be mentioned in Art. 14, whereby its strength was reduced to 434.

9. On Dec. 29th, this diluted acid was completely melted, and half of it poured into a jar with a ground stopper, and both portions exposed to the air. In the morning they were perfectly fluid; but on taking the stopper out of the jar, and dipping in it a thermometer, the acid immediately froze, beginning by forming a white coat round the ball of the thermometer, which gradually spread through the whole fluid; and at the same time the thermometer rose till it stood stationary at -5° . The cold of the acid before it began to freeze must have been about -30° , that being the temperature of a

glafs of vitriolic acid standing near it; but the thermometer which was dipped into it was five or six degrees colder, which seems to be the cause of the congelation beginning round the ball.

In the afternoon a thermometer was dipped into the other half of the acid, where, as the weather had grown less cold, it stood above a minute at -25° , without freezing; then, however, the acid froze, with the same appearance as in the morning, and at the same time the thermometer rose to -4° , and became stationary.

This acid, being left in the air with the thermometer in it, was found in the evening at -45° ; it however was not intirely frozen, being only thick as an unguent, which shews that the unfrozen part must have been of a different strength from the frozen part; but it does not appear whether stronger or weaker. The next morning it was frozen solid, though the cold was only half a degree greater.

On Jan. 16th, this acid was again tried in the same manner; it then suffered a thermometer, whose ball had been previously warmed in the hand, to be dipped into it, and remain there several minutes without freezing, though its temperature was -35° . But on lifting up the thermometer, a drop fell from its ball into the acid, which immediately set it a freezing, and it rose up to $-4^{\circ}\frac{1}{2}$.

10. On Dec. 22d, the spirit of nitre (N^o 168.) which a few days before had been diluted with snow, so as to be reduced to the strength of 411, was divided into two equal parts, and exposed to the cold. On Dec. 29th, when the temperature of the air was $-17^{\circ}\frac{1}{2}$, one of these parts was found beginning to freeze; the other was fluid, but began to freeze on dipping in a thermometer; the thermometer in both kept stationary at

-1°1. The latter was twice re melted and exposed to the cold, and both times the temperature of the frozen acid came out the same as before.

11. The white colour of the ice in these experiments seems owing only to its consisting of very slender filaments; for in some cases, where it froze slower, and where, in consequence, it shot into larger solid masses, they were transparent, and of the same colour as the acid itself. By the continuance of a sufficient cold, the acid, which by hasty freezing put on the white appearance, would become hard solid ice, but yet still retained its white appearance, owing perhaps to the filaments first shot consisting of an acid differing in strength from that which froze afterwards, and filled up the interstices.

In all these experiments, whether the ice was formed into minute filaments or solid masses, still, whenever there was a sufficient quantity of fluid matter to admit of it, they constantly subsided to the bottom; a proof that the frozen part was heavier than the unfrozen. The difference indeed is so great, that in one case where it froze into solid crystals on the surface, these crystals, when detached by agitation, fell with force enough to make a tinkling noise against the bottom of the glass.

These acids contract very much on freezing. Whenever the acid is frozen solid, the surface, instead of being elevated in ridges, like frozen water, is depressed and full of cracks. In one experiment Mr. M^r NAB, after a glass almost full of acid was nearly frozen, filled it to the brim with fresh acid; and then, after it was completely frozen, the surface was visibly depressed, with fissures one-eighth of an inch broad, extending from top to bottom. It is this contraction of the acid in freezing which makes the frozen part subside in the fluid

part; as it was found, in the undiluted acid, that the latter consisted of a stronger, and consequently heavier, acid than the former. But still the subsidence of the frozen part shews, that the ice is not mere water, or even a very dilute acid; which indeed was proved by the examination of the Liquors sent home.

The ninth and tenth articles shew, that though the acids bear being cooled greatly below the freezing point, without any congelation taking place, yet as soon as they begin to freeze they immediately rise up to their freezing point; and this point is always very nearly, if not exactly, the same in the same acid; for those acids were frozen and melted again three or four times, and were cooled considerably more below the freezing point in one trial than another, and yet as soon as they began to freeze the thermometer immersed in them constantly rose nearly to the same point.

The quantity which these acids will bear being cooled below the freezing point, without freezing, is remarkable. The diluted spirit of nitre, whose freezing point is $-1^{\circ}\frac{1}{2}$, once bore being cooled to near -39° , without freezing, that is, near 37 degrees below its freezing point. The diluted dephlogisticated spirit of nitre, whose freezing point is -5° , bore cooling to -35° ; and the dephlogisticated spirit of nitre (141) whose true freezing point is most likely -19° (*see next article*) bore being cooled to -49° : perhaps too they might have born to be cooled considerably lower without freezing, but how much does not appear. It must be observed, however, that the same diluted spirit which at one time bore being cooled to -39° , at another froze, without any apparent cause, when its cold was certainly less than -30° , and most likely not much below -18° .

12. The freezing point differs remarkably, according to the strength of the acid. In the diluted dephlogisticated and common spirit of Art. 7. and 8. the freezing point was -5° and $-1^{\circ}\frac{1}{2}$. In the dephlogisticated and common spirit of Art. 5. the decanted parts of which were stronger than the foregoing in scarcely so great a proportion as that of four to three, it seemed to be -30° and $-31^{\circ}\frac{1}{2}$. It may indeed be suspected, that as this point was determined only by pouring a small quantity of the acid into a glass, at a time when the air and glass were much colder than the acid, these decanted liquors might be cooled by the air and glass, and thereby make the freezing point appear lower than it really was: but I do not think this could be the case; for as the decanted liquors were full of small filaments of ice, they could hardly be cooled sensibly below their freezing points without freezing; and any cold, communicated to them by the air or glass, would serve only to convert more of them into ice, without sensibly increasing their cold: so that I think this experiment determines the true freezing point of their decanted part; but it must be observed, that as the decanted part was rather stronger than the rest, it is very possible that the freezing point of the undecanted part might be considerably less cold.

A circumstance which might incline one to think, that the way by which the freezing point was determined in this experiment is defective is, that the freezing point of the dephlogisticated acid N^o 27. though nearly of the same strength as that last mentioned, but rather stronger, was much less low, being only -19° . But I have little doubt that the true reason of this is, that in the former acid the strength of the decanted part, which is the part whose freezing point was tried, was found to be at least $\frac{1}{10}$ greater than that of the whole mass; whereas
in

in N° 27. the fluid part was in all probability not sensibly stronger than the whole mass; for as N° 27. was cooled only seven degrees below the freezing point, and its temperature was tried soon after its beginning to freeze, not much of the acid could have frozen; whereas the other was cooled 15 degrees below its freezing point, and was exposed for an hour or two to an air not much less cold, in consequence of which a considerable part of the acid must have frozen; so that in all probability the acid, whose freezing point was found to be -30° , was in reality $\frac{1}{4}$ part stronger than that whose freezing point was -19° .

If this reasoning be just, the freezing point of these acids is as follows:

		Freezing point.
Dephlogisticated spirit of nitre, whose strength =	.56	-30°
	.53	-19
	.437	$-4\frac{1}{2}$
Common spirit of nitre, whose strength =	.54	$-31\frac{1}{2}$
	.411	$-1\frac{1}{2}$

On the Phenomena observed on mixing Snow with these Acids.

13. On Dec. 13, snow was added to the spirit of nitre N° 168, as recommended in Art. 2. The snow was put in very gradually, and time was taken to find what effect each addition had on the thermometer and mixture, before more was added. The temperature of the acid before the mixture was -27° , and each addition of snow raised the thermometer a little, till it rose to $-1^{\circ}\frac{1}{4}$; after which the next addition made it sink to -2° , which shewed that sufficient snow had then been added. The quantity

quantity of snow used was pretty exactly $\frac{1}{10}$ of the weight of the acid, the weight of the acid being 13 oz. so that the strength of the diluted acid was reduced to ,411.

The acid before the addition of snow had no signs of freezing, its temperature being in all probability much above its freezing point; yet the snow did not appear to dissolve, but formed thin white cakes, which however did not float on the surface, but fell to the bottom, and when broke by the spatula formed a gritty sediment; so that it appears, that these cakes are not simply undissolved snow, but that the adjoining acid absorbed so much of the snow in contact with it, as to become diluted sufficiently to freeze with that degree of cold, and then congealed into these cakes. The quantity of congealed matter seems to have kept increasing till the end of the experiment.

14. On Dec. 21, an experiment was made in the same manner with the dephlogisticated spirit of nitre N^o 27. The acid began to freeze in pouring it into the jar in which the mixture was to be made, and stood stationary there at -19° , as related in Art. 6.; so that the liquor at the beginning of the experiment was white and thick, which made the effect of the addition of the snow less sensible. However, the congealed matter constantly subsided to the bottom, and the quantity seems to have continued increasing to the end of the experiment. The heat of the mixture rose to -4° before cold began to be produced, and the quantity of snow added was $\frac{1}{10}$ of that of the acid, so that the strength of the acid was reduced to ,437 by the dilution.

A very remarkable circumstance in this experiment is, that the acid, while the snow was adding, first became of a yellowish,

lowish, and afterwards of a greenish or bluish hue. This colour did not go off by standing, but continued at least ten days, during which time the acid constantly kept that colour, except when by hasty freezing it shot into small filaments, in which case it put on the white appearance which these acids always assumed under those circumstances; but once that by gradual freezing it shot into transparent ice, this ice was of a bluish colour.

It is difficult to conceive what this colour should proceed from. Spirit of nitre is well known to assume this colour when much phlogisticated and properly diluted; but one does not see why it should become phlogisticated by the addition of the snow, and still less why the dephlogisticated acid should become more phlogisticated thereby than the common acid did; for though it is not extraordinary, that a process not capable of producing any increase of phlogistication in the common acid, should make this as much phlogisticated as that, yet it is very extraordinary that it should make it more so. No notice is taken of any effervescence or discharge of air while it was assuming this colour, nor was it observed that it became more smoking thereby, or that the top of the phial in which it was kept became full of red fumes, as might naturally be expected if it was rendered much phlogisticated. These are circumstances which, considering Mr. Mc NAB's great attention to set down all the phenomena that occurred, I should think would hardly have been omitted if they had really happened.

15. It is remarkable, that in both these experiments the addition of snow produced heat, until it arrived pretty exactly at what was found to be the freezing point of the diluted acid; but that as soon as it arrived at that point, the addition of more snow began to produce cold. This can hardly be owing merely

merely to accident, and to both acids having happened to be of that precise degree of heat before the experiment began, that their heat after dilution should coincide with the freezing point answering to their new strength. The true cause seems to be as follows. It will be shewn in Art. 16. and 17. that the freezing point of these acids, when diluted as in the foregoing experiments, is much less cold than when they are considerably more diluted; and it was before shewn to be much less cold than when not diluted; so that there must be a certain degree of strength, not very different from that to which these acids were reduced by dilution, at which they freeze with a less degree of cold than when they are either stronger or weaker. Now in these experiments, the temperature of the liquors before dilution was below this point of easiest freezing, and a great deal of the acid was in a state of congelation all the time of dilution; the consequence of which is, that when they were diluted to the strength of easiest freezing, they would also be at the heat of easiest freezing; for they could not be below that point, because, if they were, so much of the acid would immediately freeze as would raise them up to it; and they could not be above it, for, if they were, so much of the congealed acid would dissolve as would sink them down to it. After they were arrived at this strength of easiest freezing, the addition of more snow would produce cold, unless this strength be greater than that at which the addition of a small quantity of snow begins to produce cold; but even were this the case, heat would not be produced, but the temperature of the acids would remain stationary until they were so much diluted that the addition of more snow should produce cold. So that, in either case, the heat of the acids, at the time that the addition of fresh snow began to produce cold, must be that of easiest freezing;

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freezing; and consequently, as this heat was found to coincide very nearly with the freezing point of these acids, after dilution, it follows that their strengths at that time could differ very little from the strength of easiest freezing.

If the temperature of the liquors at the beginning of the experiment had been above the point of easiest freezing, none of the acid would have congealed during the dilution, and nothing could have been learnt from the experiment relating to the point of easiest freezing; but the heat would have kept increasing, till the acid was diluted to that degree of strength at which the cold produced by the dissolving of the snow was just equal to the heat produced by the union of the melted snow with the acid*; after which the addition of more snow would begin to produce cold. When I recommended this method of finding the best strength of spirit of nitre for producing cold, by the addition of snow, I was not aware of any impediment from the freezing of the acid, in which case it would have been a very proper method; but on account of this circumstance it can hardly be considered as such, except when the cold of the acid at setting out is less than that of easiest freezing.

In the dephlogistified spirit of nitre the freezing points answering to the strength of ,434, ,553, and ,56, were said to be $-4^{\circ}\frac{1}{2}$, -19° , and -30° ; and the differences of -30° and -19° from $-4^{\circ}\frac{1}{2}$ are to each other very nearly in the duplicate ratio of ,126 and ,096, the differences of the corresponding strengths from ,434; which, as ,434 is the strength of easiest freezing, is the proportion that might naturally be

* In the experiment related in my observations on Mr. HUTCHINS'S Experiments, this strength was rather greater than that of easiest freezing: but whether it is so in degrees of cold exceeding that in which my experiment was tried, does not appear.

expected, and consequently serves in some measure to confirm the reasoning in this and the 12th Article.

16. After Mr. M^r Nair had diluted these acids as above-mentioned, he divided each of them into two parts, and tried what degree of cold could be produced by mixing them with snow. On January 15th, one of these parts of the common spirit of nitre was tried. It was fluid when the experiment began, though its temperature, as well as that of the snow, was $-21^{\circ}\frac{1}{2}$; but on adding snow it immediately began to freeze, and grew thick, and its heat increased to $-2^{\circ}\frac{1}{2}$; but by the addition of more snow it quickly sunk again, and at last got to $-43^{\circ}\frac{1}{2}$. During the addition of the snow, the mixture grew thinner, and by the time it arrived at nearly the greatest degree of cold, consisted visibly of three parts: the lowest part, which consisted of frozen acid, was white and felt gritty; the upper part, which occupied about an equal space, was also white, but felt soft, and must have consisted of unmelted snow; the other part, which occupied by much the smallest space, was clear and fluid. The quantity of snow added was about $\frac{2}{3}$ of the weight of the acid, and consequently its strength was reduced to, 243.

Though snow was added to the acid in this experiment as long as, and even longer than, it produced any increase of cold, yet some days after, on adding more snow to the mixture, while it was fluid, and of the temperature of $-40^{\circ}\frac{1}{2}$, the cold was increased to $-44^{\circ}\frac{1}{2}$, or 1 degree lower than before. Mr. M^r Nair did not perceive the snow to melt, though in all probability some must have done so, or no cold would have been produced.

The cause of this seems to be, that in the preceding experiment the congealed part of the acid was stronger than the

fluid part; so that, though the fluid part was not strong enough to dissolve snow in a cold greater than $-43^{\circ}\frac{1}{2}$, yet the whole acid together was strong enough to do it in a cold one degree greater.

A circumstance occurred in the last experiment which I cannot at all see the reason of; namely, a small part of the acid being poured into a saucer, before the addition of the snow, it was in an hour's time changed into solid ice, though the cold of the air, at the time the acid was poured out, was only $-41^{\circ}\frac{1}{2}$, and does not seem to have increased during the experiment.

17. On December 30, the other half of the same acid had been tried in the same manner; at the beginning of the experiment not more than one-ninth part of the acid was fluid, the rest solid clear ice; its temperature was $-34^{\circ}\frac{1}{2}$, and that of the snow nearly the same; the greatest degree of cold produced was $-42^{\circ}\frac{1}{2}$; and the quantity of snow employed was about one-eighteenth of the weight of the acid; so that the strength of the mixture was ,38. The freezing point of the acid thus diluted appears to be about $-45^{\circ}\frac{1}{2}$; for by the increase of warmth during the day-time, most of the congealed matter dissolved; but in the evening it began to freeze again, so as to become thicker, its temperature being then $-45^{\circ}\frac{1}{2}$; and the next morning it was frozen solid, its cold being one degree greater.

18. On December 12, the diluted spirit of nitre N° 139, whose strength was ,175, was found frozen, its temperature being -17 . The fluid part, which was full of thin flakes of clear ice, and was of the consistence of syrup, was decanted into another bottle, and sent back. Its strength was ,21, and was greater than that of the undecanted part in the proportion of ,21 to ,16; so that, as not much of the undecanted part was

really congealed, the frozen part of the acid must have been much weaker than the rest, if not mere water. Accordingly, during the melting of the undecanted part, the frozen particles swam at top. Mr. Mc NAB added snow to a little of the decanted liquor, but it did not dissolve, and no increase of cold was produced.

19. From these experiments it appears, that spirit of nitre is subject to two kinds of congelation, which we may call the aqueous and spirituous; as in the first it is chiefly, if not entirely, the watery part which freezes, and in the latter the spirit itself. Accordingly, when the spirit is cooled to the point of aqueous congelation, it has no tendency to dissolve snow and produce cold thereby, but on the contrary is disposed to part with its own water; whereas its tendency to dissolve snow and produce cold, is by no means destroyed by being cooled to the point of spirituous congelation, or even by being actually congealed. When the acid is excessively dilute, the point of aqueous congelation must necessarily be very little below that of freezing water; when the strength is ,21, it is at -17° , and at the strength of ,243, it seems, from Art. 16, to be at -44° . Spirit of nitre, of the foregoing degrees of strength, is liable only to the aqueous congelation, and it is only in greater strengths that the spirituous congelation can take place. This seems to be performed with the least degree of cold, when the strength is ,411, in which case the freezing point is at -1° . When the acid is either stronger or weaker, it requires a greater degree of cold; and in both cases the frozen part seems to approach nearer to the strength of ,411 than the unfrozen part; it certainly does so, when the strength is greater than ,411, and there is little doubt but what it does so in the other case. At the strength of ,54 the point of spirituous congelation

congelation is $31^{\circ}\frac{1}{2}$, and at .33 probably $-45^{\circ}\frac{1}{2}$; at least one kind of congelation takes place at that point, and there is little doubt but that it is of the spirituous kind. In order to present this matter more at one view, I have added the following table of the freezing point of common spirit of nitre answering to different strengths.

Strength.	Freezing point.	
.54	$-31^{\circ}\frac{1}{2}$	} spirituous congelation.
.411	$-1^{\circ}\frac{1}{2}$ *	
.38	$-45^{\circ}\frac{1}{2}$	
.243	$-44^{\circ}\frac{1}{2}$	} aqueous congelation.
.21	-17°	

20. In trying the first half of the dephlogisticated spirit of nitre, the cold produced was $-44^{\circ}\frac{1}{2}$. The acid was fluid before the addition of the snow, and of the temperature of -30° , but froze on putting in the thermometer, and rose to -5° , as related in Art. 7.

In trying the second part, the acid was about 0° before the addition of the snow, and therefore had no disposition to freeze. The cold produced was $-42^{\circ}\frac{1}{2}$.

As the quantity of snow added in these experiments was not observed, they do not determine any points of aqueous or spirituous congelation in this acid; but there is reason to think, that these points are nearly the same as those of common spirit of nitre of the same strength, as the cold produced in these experiments was nearly the same as that obtained by the common spirit of nitre.

* The point of easiest freezing.

On the Vitriolic Acid.

21. On December 12, the strong oil of vitriol N° 151, was found frozen, and was nearly of the colour and consistence of hogs-lard. Its temperature, found by pressing the ball of a thermometer into it, was -15° , and that of the air nearly the same; but in the night it had been exposed to a cold of -33° . It dissolved but slowly on being brought into a warm room, and was not completely melted before it had risen to $+20^{\circ}$, and even then was not very fluid, but of a syrupy consistence. During the progress of the melting, the congealed part sunk to the bottom, as in spirit of nitre: and many air bubbles separated from the acid, which, when it was completely melted, formed a little froth on the surface. As soon as it was sufficiently melted to admit of it, which was not till it had risen to the temperature of $+10^{\circ}$, the fluid part was decanted, and both were sent home to be examined.

It is remarkable, that the frozen part did not intirely dissolve until the temperature was so much increased. This would incline one to think, that the frozen part must have differed in some respect from the rest, so as to require much less cold to make it freeze; but yet I could not find that the strength of the decanted part differed sensibly from the rest.

It appeared by another bottle of oil of vitriol, which also froze by the natural cold of the air, that this acid, as well as the nitrous, contracts in freezing.

22. On December 21, when the weather was at -30° , the vitriolic acid N° 103, was diluted with snow, as directed in Art. 3. The snow dissolved immediately, and no signs of congelation appeared during any part of the process. The temperature

temperature of the acid rose only one degree before it began to sink, and the weight of the snow added was only $\frac{1}{11}$ of that of the acid, so that its strength was reduced thereby to .605; which is therefore the best degree of strength for producing cold by the addition of snow, when the degree of cold set out with is -30° . This strength is one-fifteenth part less than what I found myself, by a similar experiment, when the temperature of the acid was $+27^{\circ}$; which shews, that the best degree of strength is rather less, when the degree of cold set out with is great than when small, but that it does not differ much.

23. The acid thus diluted was divided into two parts, and the next day Mr. M^c NAB tried what degree of cold could be produced by adding snow to one of them. The temperature of the air at the time was -39° , and the mixture sunk by the process to $-55^{\circ}\frac{1}{2}$. The snow dissolved readily, and the mixture did not lose much of its fluidity until it had acquired nearly its greatest degree of cold, nor did any congealed matter sink to the bottom in any part of the process. The quantity of snow added was about $\frac{1}{10}$ of the weight of the acid, so that the strength of the mixture was about .325.

24. On January 1, thin crystals of ice were found diffused all through this mixture, the temperature of the air being $-51^{\circ}\frac{1}{2}$, but that of the liquor was not tried. As this congelation must have been of the aqueous kind, and seems to have taken place at the temperature of $-51^{\circ}\frac{1}{2}$, it should follow, that this acid had no power of dissolving snow in a cold of $51^{\circ}\frac{1}{2}$; so that it does not at first appear why a cold four degrees greater than that should have been produced in the foregoing experiment. The reason is, that at the time the mixture arrived at $-55^{\circ}\frac{1}{2}$, it appeared by the diminution of its fluidity to have contained

contained some undissolved snow; and some more was added to it after that time, which before the first of January dissolved and mixed with the acid; so that the acid in the mixture, at the time it sunk to $-55^{\circ}\frac{1}{2}$, was not quite so much diluted as that which froze on January 1. This is the reverse of what happened in the trial of the nitrous acid in Art. 15. as in that experiment the fluid part, at the time of the greatest cold, was weaker than the whole mixture together; but it must be considered, that *that* mixture contained much congealed acid, as well as undissolved snow, whereas *this* contained only the latter.

25. On January 1, snow was added to the other half of the acid diluted on December 21. The cold produced was much greater than before, namely $-68^{\circ}\frac{1}{2}$; this seems to have proceeded, partly from the air and materials having been 12 degrees colder in this than in the former experiment, and partly from the snow having been added faster, so that the mixture arrived at its greatest degree of cold in 20', whereas it before took up 46'. Another reason is, that the former mixture was made in too small a jar, in consequence of which it was poured into a larger before the experiment was completed, whereby some cold was lost. The quantity of snow used in this experiment was less than in the former, so that the strength of the acid after the experiment was about ,343. The mixture also grew much thicker, and had a degree of elasticity resembling jelly; but whether this was owing only to more snow remaining undissolved, or to any other cause, I cannot tell.

26. Great as the foregoing degree of cold is, Mr. M^r NAB, on February 2, produced one much greater. In hopes of obtaining a greater degree of cold by previously cooling the materials, he cooled about seven ounces of oil of vitriol, whose strength was ,629, that is, rather stronger than the foregoing,

by placing the jar in which it was contained in a freezing mixture of oil of vitriol and snow; the snow intended to be used was also cooled by placing it under the vessel in which the freezing mixture was made. As soon as the acid in the jar was cooled to the temperature of $-57^{\circ}\frac{1}{2}$, a little of the snow was added, on which it immediately began to freeze, and rose to -36° ; but in about 40 minutes, as the jar was still kept in the freezing mixture, it sunk to -48° ; by which time it was grown very thick and gritty, especially at bottom. More of the cooled snow was then added, which in a short time made it sink to $-78^{\circ}\frac{1}{2}$, and at the same time the thickness and tenacity of the mixture diminished; so that by the time it arrived at the greatest degree of cold, very little thickness remained.

It is worth inquiring, what was the reason of the greater degree of cold produced in this than in the preceding experiment? It could not be owing to the materials being colder; for at the time of the second addition of snow, at which time the experiment may be considered to have begun, the acid was not colder than at the beginning of the preceding experiment, and the snow in all probability not much colder. It could not be owing neither to the jar having been kept in the freezing mixture: for though that mixture was three or four degrees colder than the air in the preceding experiment, yet the acid in the jar, before it acquired much addition of cold, would be robbed of its cold faster by the mixture than it would by air of the same temperature as that in the preceding experiment. Neither could it proceed from any difference in the strength of the acid; for what difference there was must have done more hurt than good. The true reason is, that the acid was in a state of congelation: for as the congealed acid united to the snow and became fluid by the union, it is plain, that cold must have been produced both
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by the melting of the snow and by that of the acid; whereas, if the acid had been in a fluid state, cold would have been produced only by the first cause, and consequently a greater degree of cold should be produced in this experiment than in the former. The only inconvenience attending the acid being in a state of congelation is, that in all probability it does not unite to the snow so readily as when in a fluid state; but the difference seems not material, as the cold was produced, and the materials melted, in 5 minutes.

27. The day before, Mr. M^c NAB, by adding snow to some of the same acid in the usual manner, when the cold of the materials was -46° , produced a cold of only -66° .

28. In these four last experiments the acid was reduced, by the addition of the snow, to the strengths of ,325, ,343, ,403, and ,334; and the cold produced in them was before said to be $-55^{\circ}\frac{1}{2}$, $-68^{\circ}\frac{1}{2}$, $-78^{\circ}\frac{1}{2}$, and -66° ; whence we may conclude, that these are nearly the points of aqueous congelation answering to the foregoing strengths; only it appears, from what was said in Art. 24. that the strengths here set down are all of them rather too small.

Though it is certain that oil of vitriol is capable of the spirituous congelation, and though it appears, both from the foregoing experiments and from some made by the Duc D'AYEN* and by M. DE MORVEAU †, that it freezes with a less degree of cold when strong than when much diluted, it is not certain whether it has any point of easiest freezing, like spirit of nitro, or whether the cold required to freeze it does not continually diminish as the strength increases, without limitation; but the latter opinion is the most probable. For the Duc D'AYEN's and

* Diction. de Chym. par MACQUER, 2^de edit.

† Nouv. Mém. de l'Académ. de Dijon, 1782, 1^{er} semestre, p. 68.

M. DE MORVEAU's acids, which, as they were concentrated on purpose, were most likely stronger than Mr. M^c NAB's, froze with a cold less than zero of FAHRENHEIT; whereas the freezing point of Mr. M^c NAB's undiluted acid, whose strength was .98, was -15° , and that of the diluted acid, whose strength was .629, was -36° ; and when the acid was more diluted, it was found to bear a much greater cold without freezing. It appears also, both from Art. 21. and from M. DE MORVEAU's experiment, that during the congelation of the oil of vitriol, some separation of its parts takes place, so that the congealed part differs in some respect from the rest, in consequence of which it freezes with a less degree of cold; and as there is reason to think from Art. 21. that these two parts do not differ much in strength, it seems as if the difference between them depended on some less obvious quality, and probably on that, whatever it is, which forms the difference between glacial and common oil of vitriol. The oil of vitriol prepared from green vitriol, has sometimes been obtained in such a state as to remain constantly congealed, except when exposed to a heat considerably greater than that of the atmosphere, whence it acquired its name of *glacial* *. It is not known indeed upon what this property depends, but it is certainly something else than its strength; for oil of vitriol of this kind is always smoking, and the fumes it emits are particularly oppressive and suffocating, though very different from those of the volatile sulphureous acid. On rectification likewise it yields, with the gentlest heat, a peculiar concrete substance, in the form of saline crystals; and after this volatile part has been driven off,

* Mém. de l'Académ. des Sc. 1738, p. 288.

the remainder is no longer smoking, and has lost its glacial quality*.

On the Mixture of Oil of Vitriol and Spirit of Nitre.

29. This mixture is not so fit for producing cold by the addition of snow, as oil of vitriol alone; for the cold obtained did not exceed $-54^{\circ}\frac{1}{2}$, in either of the experiments tried with it. The point of spirituous congelation of this mixture, when diluted with somewhat more than one-tenth of its weight of water, is about -20° , and is much lower when the acid is considerably more diluted: but as the Society will most likely have less curiosity about the disposition to freeze of this mixture than of the simple acids, I shall spare the particulars.

On the Spirit of Wine.

30. The rectified spirits N° 8. were diluted with snow, in the same manner as the other liquors; but were found not to want any, as the first and only addition of snow produced cold. The quantity added was about $\frac{1}{10}$ of the weight of the spirit.

31. The spirit thus diluted was divided, like the other liquors, into two parts, and each tried separately. The first was at -45° , before the addition of the snow, and was sunk by the process to -56° . The snow, even at the first addition, did not dissolve well, so that the spirit immediately

* CRELL's Neu. Entdeck. in der Chemie, Th. 11. p. 100. Th. 12. p. 242, &c. and Annalen, 1785, St. 5. p. 438, &c.

became full of white spots*, and grew thick by the time it arrived at its greatest degree of cold. After standing some hours, the mixture rose to the temperature of -39° , and was grown clear, but yet was not limpid, but of the consistence of syrup. No cold was produced by adding snow to it in that state, though it appeared that its point of aqueous congelation was at least 6 degrees lower than its temperature at that time†; which seems to shew that spirit of wine has scarce any power of dissolving snow when it wants even 6 degrees of its point of aqueous congelation, and therefore is another instance that snow is dissolved much less readily by spirit of wine than by the nitrous and vitriolic acids.

32. In trying the other part of the diluted spirits, the cold produced was only $-47^{\circ}\frac{1}{2}$, the cold set out with being -37° .

33. It appeared by the diluted spirit of wine N° 143. which on December 12 froze by the natural cold of the atmosphere, and was treated in the same manner as the diluted spirit of nitre, that when highly rectified spirit of wine, such as N° 8. is diluted with $1\frac{4}{5}$ its weight of water, its point of aqueous congelation will be at -21° . The congealed part of the spirit was white like diluted milk, and even the decanted part, which was full of thin films of ice, had a milky hue. The fluid part was stronger than the rest, and no increase of cold was produced by adding snow to some of it, both of which are marks of aqueous congelation.

* This was not the case during the above-mentioned dilution of the spirits; but the cold was 16 degrees less in that experiment than in this.

† On account of the dilution which the spirits suffered by the melting of the snow which remained undissolved at the time of the greatest cold, its point of aqueous congelation was no longer so low as -56° ; but it still was not less than $-45\frac{1}{2}$, as in the evening it was found at that temperature, without much congealed matter in it.

Though the foregoing experiments confirm the truth of what I said, in the account of Mr. HUTCHINS's experiments, concerning the cause of the cold produced by mixing snow with different liquors, and intirely clear up the difficulty relating to it which I mentioned in Art. 1. yet several questions may naturally occur; such as, why the cold produced by the oil of vitriol was so much greater than that obtained by the spirit of nitre, notwithstanding that in warmer climates the nitrous acid seems to produce more cold? and why the cold produced by the nitrous acid, notwithstanding its previous dilution, which might naturally be expected to be of service, was not greater than has been obtained by other persons without that precaution? But as this would lead me into disquisitions of considerable length, without my being able to say any thing very satisfactory on the subject, I shall forbear entering into it. I will only observe, that in most of the foregoing experiments, Mr. Mc NAB would probably have produced more cold, if he had added the snow faster. We ought not, however, to regret that he did not, as its effects on the acids would then have been less sensible.

The natural cold, when these experiments were made, is remarkable; as there were at least nine mornings in which the cold was not less than that of freezing mercury; four in which it was at least eight degrees below that point, or -47° ; and one in which it was -50° . Whereas out of nine winters, during which Mr. HUTCHINS observed the thermometer at Albany Fort, there were only twelve days in which the cold was equal to that of freezing mercury, and the greatest cold seems to have been -45° . I cannot learn whether the last winter was more severe than usual at Hudson's Bay; or whether Henley-House is a colder situation than Albany, which
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may perhaps be the case; for though it is only 130 miles distant from it, yet it stands inland, and to the W. or S.W. of it, which is the quarter from which the coldest winds blow.

Mr. Mc NAB's original account of the experiments which furnished the materials of this Paper, having been thought too long to be printed in detail, is deposited in the Archives of the Society.

END OF PART I. OF VOL. LXXVI.